Empirical Essays on The Economics of Education and Pay

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by

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Declaration

I hereby declare that the work in this thesis was carried out in accordance with the Regulations of the University of Warwick. I have not used before or published any material contained in this thesis, and it has not been submitted for a degree at another university. Chapters 2 and 3 are single authored. Chapter 4 is joint authored with Fabien Postel-Vinay and Hélène Turon, University of Bristol. Chapter 3 developed from my MSc dissertation, however the chapter itself is a substantial development of that idea; specifically: the treatment of the economic and econometric modelling is much advanced, the substantiation of the hypothesis proposed and exploration of alternative hypotheses is much more rigorous, a much greater literature is incorporated, and a substantially larger dataset is used.

Abstract

This thesis consists of three stand-alone papers which address different questions regarding the economics of education and pay.

The Effect of Free Pre-school Education on Children's Subsequent Academic Performance: Empirical Evidence from England (Chapter 2)

This chapter address the question of whether starting formal education part-time at age three has a positive effect on children's academic attainment when they reach age 7 and whether this depends on the sector providing the early education. Using a panel of English Local Education Authorities I initially utilise the fact that mandatory provision of free early education for 3-year olds was introduced at different times according to the deprivation of the LEA and then estimate effects separately for more and less deprived LEAs. Exploiting the time dimension of the panel dataset, I am able to control for time-invariant unobserved heterogeneity at the LEA level that may confound estimates from other British cohort studies which rely primarily on cross-sectional variation. I find that early education in public sector nursery and primary schools in the more deprived LEAs has a small positive effect on attainment in reading and writing. These findings suggest that state maintained nursery settings are more effective than private sector providers of early education, especially in more deprived LEAs.

The Causal Effect of Education on Wages Revisited (Chapter 3)

In this chapter I estimate the causal effect of education on wages comparing estimates that are derived using variations in schooling associated with (a) early smoking behaviour, and (b) the raising of the minimum school leaving age. Earlier research using similar methods covers a wide range and my work is motivated by the concern that what is sometimes claimed as *the* return to education is only the return for a specific group and this might be rather different to the average return to education in the population. Each of my instruments estimates a 'local average treatment effect' and I analyze the extent to which these differ and which is more appropriate for drawing conclusions about the return to education in Britain. I implement each instrument on the same data from the British Household Panel Survey, and use the over-identification to test the validity of my instruments. I also exploit the dual sources of exogenous variation in schooling to derive a further IV estimate of the return to schooling. I find that each of my IV estimates of the return to education are not significantly different to each other (approximately 12%) and are substantially higher than the Ordinary Least Squares estimate (4.6%).

The Lifetime Public Premium in Earnings: The View from Europe (written with Fabien Postel-Vinay and Hélène Turon) (Chapter 4)

The focus of most of the current literature on public-private pay inequality is on differences in earnings levels, however the public-private differences are equally marked in terms of earnings mobility, earnings dispersion and job loss risk. Forward-looking agents care about earnings and job *mobility* as well as earnings levels, thus an assessment of the existence of a "public premium" should be based on measures of the lifetime value of employment in either sector. Using data from the European Community Household Panel survey, we evaluate the difference in lifetime value of employment in the public and private sector, taking into account differences in average earnings, earnings dispersion and earnings persistence. In addition to considering the effect of observed individual characteristics, such as education and labour market experience, the estimation strategy allows for unobserved heterogeneity – for example in terms of "public service motivation" – to influence the dynamics of individuals' employment and earnings patterns. The common format of the ECHP permits the analysis to be carried out for six different European countries – Germany, The Netherlands, France, Italy, Spain and Portugal. This is first time this modelling strategy has been applied to European data, affording an international perspective on public-private pay inequalities.

Chapter 1

Introduction

In this thesis I address three separate economics questions, concerned with the formation of human capital and how it is rewarded in the labour market. My interest is in how education affects individual's outcomes, both in the short and longer-term, and the implications this has for policy. I also assess the inequalities between the public and private sectors of the labour market over a working lifetime.

The first main chapter (chapter 2) addresses the question of whether starting formal education part-time at age three has a positive effect on children's academic attainment when they take their first standard assessment tests at age seven. Moreover, does the answer to this question depend on which sector provides the pre-school education? I have constructed a panel dataset of English Local Education Authorities' pre-school attendance rates and subsequent test scores. To estimate the effect of free early education places for 3-year olds on the subsequent scores, initially I utilize the fact that the policy making mandatory the provision of free early education was introduced at different times according to the deprivation of the LEA. The 65 most deprived LEAs had the policy introduced one year earlier than the remaining 85 LEAs. In order to assess whether the effect is different for these poorest LEAs, I then estimate effects separately for the more and less deprived LEAs. Exploiting the time dimension of the panel dataset, I am able to control for time-invariant unobserved heterogeneity at the LEA level that may confound estimates from other British cohort studies which rely primarily on cross-sectional variation. The main finding is that early education in public sector nursery and primary schools in more deprived LEAs has a small positive effect – approximately 1/3 of a standard deviation of the results distribution – on attainment in reading and writing. These findings suggest that state maintained nursery settings are more effective than private sector providers of early education, especially in more deprived LEAs. This has important implications for the Government's early years education policy, suggesting that expansion of state provision, rather than 'contracting out' of early education to the private sector, would have a beneficial effect on children's human capital formation, as measured by their early test scores.

Having looked at how early education affects subsequent educational attainment, the next chapter turns to the question of how education is rewarded in the labour market. In this chapter I estimate the causal effect of education on wages, comparing ordinary least squares estimates with two alternative instrumental variable estimates: one exploits variation in schooling associated with early smoking behaviour, the other uses the raising of the minimum school leaving age. There is a large body of work in this area, and various instruments have been used as researchers attempt to derive an estimate of the return to education, purged of the bias in OLS. My work is motivated by the concern that what is sometimes claimed as *the* return to education is only the return for a specific group – a 'local average treatment effect' (LATE) – and this might be rather different to the average return to education in the population. Each of my instruments estimates a LATE and I analyze the extent to which these differ and which is more appropriate for drawing conclusions about the return to education in Britain. Raising the school leaving age affected only those who had wanted to leave school early and therefore this estimate captures the effect of additional schooling for those at the bottom of the schooling distribution who were forced to stay in school longer. In contrast, I find that early smoking affects the schooling decisions of individuals across the whole distribution, thus I interpret this estimate as closer to the average return to additional education. I implement each instrument on the same data from the British Household Panel Survey, and use the over-identification to test the validity of my instruments. I also exploit the dual sources of exogenous variation in schooling to derive a further IV estimate of the return to schooling. I find that both instruments are valid and that the OLS estimate is considerably downward biased (around 4.6%) compared with the IV estimates of 12.9% (early smoking), 10.2% (RoSLA) and 12.5% (both instruments).

The final main chapter analyses the question of whether public sector workers enjoy an earnings premium in a number of different European countries. At present, the majority of the literature on public-private pay inequality concentrates on differences in earnings levels and whether observed differences can be explained by non-random sorting of workers into employment sectors. Comparisons of cross-sectional earnings levels, even after controlling for selection, give an incomplete picture if the sectors also differ with regard to earnings dispersion and earnings mobility. We show that for the countries that we study – Germany, The Netherlands, France, Italy, Spain and Portugal – the public and private sectors differ in terms of earnings dispersion and mobility, and also with respect to job mobility. Forward-looking agents care about earnings and job *mobility* as well as earnings levels, thus a more complete assessment of the existence of a "public premium" should be based on measures of the lifetime value of employment in either sector. To this end, we use data from the European Community Household Panel survey to evaluate the difference in the lifetime value of employment in the public and private sector. The modelling strategy takes into account cross-sector differences in average earnings, earnings dispersion and earnings persistence. The estimation allows observed individual characteristics, such as education and labour market experience, to influence the dynamics of individuals' employment and earnings trajectories. In addition, the model allows for the effects of two different dimensions of unobserved heterogeneity: firstly with regard to labour market mobility – for example in terms of "public service motivation" or propensity to be unemployed; secondly with regard to individual patterns of earnings levels and mobility. The common format of the ECHP allows us to apply the same modelling strategy to all six countries. This is the first time that this modelling strategy has been applied to European data, affording an international perspective on public-private pay inequalities.

Chapter 2

The Effect of Free Pre-school Education on Children's Subsequent Academic Performance: Empirical Evidence from England

2.1 Introduction

In modern society we need to decide what is the right age at which to start educating our children – in most countries the answer is deemed to be between the ages of 4 and 7. But is this the best strategy? Moreover, does the answer depend on whether the earliest education is provided through the state schools system or contracted out to private nurseries and other private sector settings? This paper sheds light on these questions, exploiting the introduction of a policy to provide free early education places for all 3-year olds in England.

There are strong theoretical motivations for the government to be involved as a provider in the early education sector. Both equity and efficiency arguments plus child development and psychology literature point to the importance of early education for later academic and other socio-economic achievements. Since being elected in 1997, the New Labour government has implemented a number of policies designed to improve the life chances of all children, particularly those with poor initial endowments, and also to allow parents greater access to the labour market through improving childcare availability. Indeed, the first National Childcare Strategy (1998)[1]

[1] See the First National Childcare Strategy: www.surestart.gov.uk/_doc/0-BB628F.doc.

had these explicit dual aims of improving child outcomes through early experiences and allowing more parents to return to the labour market – and thus improving child incomes, bringing the benefits associated with parental employment and higher family income.

Part of this wider policy agenda has been the introduction of free part-time early education places for all children in England, initially just for all 4-year olds but subsequently extended to all 3-year olds. An early education place consists of a minimum of five sessions of play-based learning per week, each session lasting two-and-a-half hours, for 33 weeks of the year. These places can be accessed at a range of settings and parents can obtain a place for their child at no formal cost to themselves. The policy to introduce these free places for all 4-year olds was introduced in 1998, and at least 96% of all 4-year olds in England were accessing a free early education place by 1999 – the majority of which were in maintained nursery schools or nursery, reception or infant classes in maintained primary schools[2]. Places can be taken in the private sector in which case these providers are paid by the Government via the Nursery Education Grant (NEG).

The Government pledged to extend provision to include 3-year olds, however since the maintained schools sector capacity is filled to a large extent by 4-year olds, much of the expansion has been borne by the 'other' sector – comprising private, voluntary and independent providers and a small number of maintained settings other than nursery and primary schools – and paid for via the NEG. This extension to 3-year olds was introduced in 1999-2000 initially only in the 65 most disadvantaged Local Education Authorities. This was then fully rolled out to all LEAs in 2000-2001, with the aim of providing a free place for all 3-year old children by September 2004.

In this paper I will address the question of whether this expansion of free early education for 3-year olds – largely in the private sector but paid for by the Government – has had a positive impact on educational attainments at Key Stage 1 (KS1). As this is the first point at which children take standard national assessments[3], KS1 results are a useful benchmark, moreover they have been shown to have predictive power for later academic outcomes and are therefore an early indicator of potential future attainment[4]. Using data on each LEA's take-up rate of free early education places in each sector in each year from 1998-2003, and linking this with the results of KS1 assessments in the LEA when the same cohort of 3-year olds took their tests, I

[2]According to figures from "Statistics of Education: Provision for children under 5, January 2002", Tables 3 and 8, and National Statistics population estimates.

[3]See Table 2.1 (on p.14) for an outline of how the Key Stages relate to age and school year.

[4]See Sammons *et al.* (1995).

create a 5-year panel of data at the level of the LEA[5]. Initially I look at a simple indicator for the policy being in operation and whether this has any effect on overall results in KS1 reading, writing and maths. I then look in more detail at how the expansion in the proportion of 3-year olds attending early education has effected LEA level results in these subjects. In addition, I allow the impact of the policy to differ according to whether the LEA was one of the poorer LEAs that were in the first phase of the policy roll out (the 'pathfinder' LEAs), which allows me to assess whether the targeted LEAs have benefited more. However, allowing differential impacts inevitably weakens the identification strategy as it reduces one source of variation in the explanatory variable of most interest.

The results indicate that there was little or no impact on reading, writing or maths, of the introduction of the free early education place policy. Looking in more detail at the effect of the increasing attendance of private sector settings by 3-year olds I find no significant effects and this continues to be the case when the impacts are allowed to differ according to whether the LEA was in the first phase of the roll-out. However evidence is found to support the conclusion that, in the poorer LEAs, increasing attendance by 3-year olds in maintained nursery and primary schools has a positive effect on the proportion of children attaining the expected level in KS1 reading and writing, while attendance of these settings by 4-year olds in the better off LEAs is associated with a larger positive effect on the proportion of children attaining the highest level in KS1 reading and writing. This is in line with the findings of the Effective Provision of Pre-school Education (EPPE) project which studied the attainments of 3,000 children in England over a similar time-period. My contribution is to examine whether the implementation of the policy at the aggregate level translates to improved outcomes within the LEA as a whole when the children first sit standard assessments – or whether any effect is mitigated by the first two years of primary school.

The paper proceeds as follows: Section 2.2 outlines the theoretical arguments for the government's involvement in the early education sector; Section 2.3 reviews the current literature on the effects of early education on later scholastic and socio-economic outcomes. Section 2.4 describes the background to the free early education place policy, its historical context, and what it entails. Section 2.5 details the model, while Section 2.6 outlines the data and it's construction before describing the data. Section 2.7 presents the results of the estimation and their implications before Section 2.8 concludes.

[5]It is a 5-year rather than 6-year panel due to data unavailability in 2002, see data section.

2.2 Why Does the Government Provide Early Years Education?

There are strong motivations for the government to be involved in the pre-school childcare and early education market. The theoretical case for government involvement derives from both equity and efficiency concerns, and is supplemented by an increasing body of empirical evidence both from the US, and to a lesser extent the UK, suggesting that early education programs can have a significant positive effect on children's academic and social outcomes both in the short- and medium-to-long term, especially for children from disadvantaged backgrounds.

From an equity point of view, the government may wish to compensate individuals who have poor socio-economic outcomes and can do so by intervening either to adjust final outcomes or to adjust initial endowments. Aside from cost and efficiency concerns, a further problem with attempting to compensate individuals through altering final outcomes is that this creates severe moral hazard issues – and indeed poverty and unemployment 'traps'. Intervening early to compensate individuals who have unequal endowments in terms of cognitive and non-cognitive skills, environments and opportunities, avoids such moral hazard problems.

It is also argued that market failures – information failures and externalities – can be addressed by the government through involvement in the early education sector. Evidence shows that parents are consistently poor at judging the quality of nurseries and childcare or knowing what factors make for good pre-school settings (Blau and Currie, 2004). This in itself does not justify government provision in this area, however, this informational asymmetry is compounded by the failure of parents to recognise and fully capitalise the value of early education for their children which is likely to lead to an under-investment. This is particularly costly to society as well as the individuals, since there are considerable externalities associated with early education in terms of the human capital of the workforce, social cohesion, and even reduced crime[6].

Moreover, though childcare and early education are slightly different areas, they are necessarily linked. Whilst early education – particularly for disadvantaged children – attempts to address differences in endowments and enhance future prospects, settings providing early education are also providing a substitute for traditional childcare, allowing both parents or the one

[6]Interventions providing pre-school education in the US, such as the Perry Pre-School Program, the Carolina Abecedarian Program, the Syracuse Pre-School Program and Head Start, have had a large degree of success in improving medium- to long-term outcomes for children from disadvantaged families; see Waldfogel, 1999; Currie, 2001, for excellent reviews of the benefits of such programs. In the UK, the EPPE project has demonstrated that pre-school is beneficial for the whole population and that disadvantaged children particularly benefit – see Melhuish, 2004.

parent in a single parent family to enter the labour market. This is a considerable motivation behind the current UK Government's policy to provide early education – it is part of a package of measures designed to allow parents to return to work and lift themselves and their families out of poverty[7].

While there are motivations from equity, market failure and externality arguments, these would to some extent be moot if it were not for efficiency considerations with respect to early investments in children – if they are not effective in improving outcomes the arguments in their favour would be much less cogent.

Initial work on human capital (Becker, 1964) treated 'ability' as a fixed genetic feature of which individuals have different endowments. More recent work, drawing on the child development literature, has advanced a more nuanced understanding of cognitive and non-cognitive abilities, how they interact, how they can be developed and the extent to which they are malleable (Shonkoff and Phillips, 2000; Ridley, 2003). However, even if we ignore these aspects, the efficiency arguments for early investment are supported by the fact that investments in children are harvested over a longer time horizon than later investments, making them more cost-effective, *ceteris paribus.*

Moreover, of much greater significance for the efficiency case is recent research, primarily by Heckman and co-authors (Cuhna *et al.*, 2005; Carneiro and Heckman, 2003), into the impacts on child outcomes of investments in children made at different times during childhood. This research suggests that investments made early in the child's lifetime are of much greater benefit in terms of medium- to longer-term outcomes – schooling and socio-economic – than investments made later. This is because of the dynamic and synergistic nature of human capital development which exhibits self-productivity and complementarity. Skills attained early in the life cycle facilitate the attainment of further skills later and also increase the productivity of later investments – as Carneiro and Heckman put it "skill and ability beget future skill and ability" (2003, p.1). Studies of child development (Shonkoff and Phillips, 2000; Shore, 1997) emphasize that there are critical stages for the development of different types of abilities and that if the opportunity for the formation of these abilities is missed then attempts to rectify this at a later stage are problematic and expensive (Carneiro and Heckman, 2003). Deficits in cognitive and non-cognitive skills emerge early as the result of differing inherited characteristics but crucially also differences in experiences in the home and early environments which affect the development of

[7]See the First National Childcare Strategy: www.surestart.gov.uk/_doc/0-BB628F.doc

skills and the realisation of the potential of initial endowments. If uncorrected these deficits lead to low skilled adults with the opportunity for correction substantially reduced once the critical stages in childhood are passed. This implies that there is no trade-off between efficiency and equity when it comes to early investments whereas there may be for investments made at a later stage (Cunha *et al.*, 2005). It is noted however that investment later in childhood can still be productive and it is not the case that reaching a certain age represents a cut-off point after which skills cannot be developed – children show great resilience to recover from poor initial environments or the absence of positive experience (Waldfogel, 2007; Yaqub, 2002). However, as children age it becomes increasingly expensive to effectively remediate problems.

In sum, there are strong theoretical arguments that investments made early in childhood are much more efficient than those made later in childhood and later in life, and these arguments are supported by numerous empirical studies.

2.3 Current Literature

There are several excellent reviews of early intervention programs that have been in operation in the US (see Blau and Currie, 2004; Carneiro and Heckman, 2003; Melhuish, 2004; Waldfogel, 1999; Currie, 2001). The programs themselves generally fall into two categories: (a) small scale, intensive interventions of high quality and high cost; and (b) larger scale, public programs, lower cost and intensity. In each case the programs are targeted at children from disadvantaged families.

Few small scale intervention programs have been designed in such a way as to allow objective evaluation. However, there are a small number that use a randomised design and are relatively free of attrition and also follow children for a sufficient length of time to allow assessment of more than just short-term effects. Though the treatment and control group sizes in these small scale studies are typically less than 100 children, they can shed light on the effectiveness of early interventions in children's lives. The programs generally involve an enriched part- or full-day, centre-based pre-school programme, for at least two years and in some cases more than two years. The pre-schools are implemented by highly trained staff with a high teacher/student ratio. The larger scale programs, the most famous being Head Start, similarly involve a part-time enriched pre-school and quality family services for participating children, though at a lower cost than the more intensive small scale programs.

Evaluating these various programs there has emerged something of a consensus that the small scale targeted interventions show a clear benefit for disadvantaged children. These programs can increase test scores in the short term, though there is less evidence that they can actually improve cognitive ability (as measured by IQ) in the long run. Nevertheless they do improve measured school success in terms of special educational needs, grade completion, high-school graduation and likelihood of going to college. Importantly, studies into adulthood have shown that the improved scholastic success of these programs is followed by improved employment, earnings, social engagement and reduced crime.

The evidence in support of positive effects of larger scale programs is less conclusive. Kresh (1998) evaluates 30 years of research into the effects of Head Start and concludes that while the immediate effects are substantial they are less evident in the longer term. This though is partly because there have been very few well designed studies of the longer term effects (Currie, 2001). Two studies (Garces *et al.*, 2000; Oden *et al.*, 1996) that do follow Head Start children into adulthood find longer term beneficial effects on college attendance, earnings and criminality. Similarly Reynolds et al. (2000, 2001) find that the effects of the large-scale Chicago Child-Parent intervention on educational attainment, social development and crime, persisted up to the age of 20. For the larger scale programs, there is consensus that in the short term there are certainly cognitive development gains witnessed in test scores, though these appear to 'fade out' after a number of years; however benefits in school, social and behavioural achievement remain.

Given that the smaller scale programs are more highly funded, tightly controlled and run by more highly trained staff, it is not that surprising that they produce greater effects (Blau and Currie, 2004). However, Melhuish (2004) concludes that there are still substantial worthwhile benefits of the larger scale programs, with effects depending on the population and context – for example where crime is endemic, the large scale pre-school programs are effective in reducing criminality.

While there is thus a body of robust evidence from the US that high quality targeted interventions are effective in producing short and medium-to-long term benefits to disadvantaged children, there have been fewer studies evaluating the effect of pre-school programs for the child population in general. As it is my aim to look at both the effects on the more disadvantaged areas and the wider population, it is important to place my contribution in the context of the effect of pre-school education on all children as well as the disadvantaged in particular.

There are a small number of studies from both sides of the Atlantic that can shed some light on this area. Two recent longitudinal studies in the US have found significant positive effects. Magnuson *et al.* (2007) use data from the Early Childhood Longitudinal study, a nationally representative cohort of 12,800 US children, and find that after controlling for child and family characteristics, the children who attended a pre-school centre had developed better literacy and maths skills at the age of 6, with greater and more lasting effects found for the more disadvantaged. Similarly the National Institute of Child Health and Human Development carried out its Study of Early Child Care (NICHD-SECC) using longitudinal data on a cohort of 1,300 children in the US, finding that after controlling for background factors, attending a pre-school between 3 and 5 years of age increases cognitive scores at age 5. However it was also noted that the quality of the pre-school experience was crucial in determining the effects – there is not an unqualified positive effect of any pre-school *per se* (Waldfogel, 2007).

Specifically looking at the effects of expansion of provision, Cascio (2004) exploits differences in the timing of the introduction of funding for kindergartens across the US in the 1960s and 1970s to estimate the effect of public pre-school attendance on scholastic success. She finds evidence that white children who were near age 5 at the time of the reforms were 20% less likely to be retained in a grade, while for racial minorities this reduction was between 30% and 40%, and concludes that more disadvantaged children benefit from government sponsored pre-schools.

Similarly Berlinski *et al.* (2006) look at the effect of a large expansion of the pre-primary infrastructure in Argentina, aimed at increasing pre-school attendance, on subsequent school achievement. It is found that there is a significant positive effect of a year of pre-primary schooling on Spanish and Mathematics scores at the end of the third grade (age 9), of approximately 0.23 standard deviations of the distribution of results. The pre-primary schools in Argentina are almost always attached to a primary school, so this study is similarly looking at the effects of state sponsored pre-school education on early outcomes.

Levin and Schwartz (2007) look at whether vouchers should be given to parents to allow them to go public or private in their pre-school, through an experiment in Georgia. They conclude that allowing choice of establishment is indeed better, though their criteria are less focused on academic achievement.

In the UK there have been a number of cohort studies which provide data rich enough to investigate the effect of pre-school experience on later outcomes.

Jowett and Sylva (1986) using a small sample of working-class children who started school in September 1978, and controlling for child and family background characteristics, found that children attending nursery education performed better in primary school across a range of cognitive and non-cognitive dimensions than those who attended a playgroup.

The 1970 British Cohort Study collected a wide range of information on a sample of approximately 8,500 children born in one week of April 1970. Subsequently just over half of these children attended some sort of centre-based pre-school education, and these children were found to have higher cognitive attainment at ages 5 and 10 than those who had no pre-school experience, after controlling for socio-economic factors and maternal education (Osborn and Milbank, 1987). A positive effect was found for all types of pre-school settings compared to none, and there is a slight suggestion that nursery education had slightly greater benefits for disadvantaged children. However, Feinstein *et al.* (1998) have also analysed the British Cohort Study of 1970 and the 1958 National Child Development Study – which similarly follows a cohort of children born in Britain in one particular week in 1958. They find that while pre-school has a positive effect on cognitive abilities up to the age of 11, their analysis of the 1970 cohort suggests pre-school has a negative effect on vocabulary when the children were 5, and reading skills when the children were 11. Given the contrasting conclusions from these studies using the same data, it is with caution that we conclude the positive effect of nursery education on later child outcomes from this evidence.

More recently, Goodman and Sianasi (2005) have exploited the 1958 NCDS data and the longer time horizon to estimate the effect of pre-school experience on later child and adult outcomes. Using a sample of 12,500 children and after controlling for child, parent, family and neighbourhood characteristics, attending any pre-compulsory education (includes early school start as well as a pre-school centre) was found to have a positive effect on cognitive skills at age 7, which last through to age 16, though diminishing in size. Attending a nursery or playgroup in particular raised test scores, though this impact proved to be short-term.

While these UK studies provide some evidence of the effects of pre-school, it is difficult to extrapolate conclusions concerning the effects of attending a pre-school in the 1960s or even the 1970s to the present day, given changes over recent decades in educational practice and pedagogy, particularly in the early education sector.

To specifically investigate the impact of current pre-school experiences on young children's intellectual and social development, the UK government launched the Effective Provision of Pre-School Education (EPPE) study in 1997. A sample of 3,000 children, from a range of social backgrounds, has been followed from the age of 3 and continues to be followed through their school careers. These children attended a range of different pre-school settings and there was also a sample (around one-tenth of the size of the main sample) of 'home' children who had no pre-school experience at all, in order to be able to estimate the effect of pre-school compared to none, as well as the effects of different types of setting. Multi-level modelling is used to evaluate the effects of different types of pre-school, controlling for background factors such as birth weight, gender, parental qualifications/occupations, home language and the home learning environment. However, despite rich data, there is no experiment or natural experiment exploited in the study.

Briefly, the EPPE study finds that pre-school attendance by 3- and 4-year olds improves children's cognitive development and social behaviour on school entry, controlling for a rich set of background characteristics. Moreover, starting pre-school earlier is associated with positive intellectual gains, though there is no evidence that full day attendance had any greater impact than half day – the majority of children attended pre-school only half day. In terms of the additional effectiveness for disadvantaged children, the research suggests that pre-school can be an effective intervention for the reduction of special educational needs for the most disadvantaged and that high quality pre-school when aged 3 and 4 can improve reading skills and reduce prevalence of anti-social/worried behaviour. Whilst not claiming to eliminate initial disadvantages, pre-school can mitigate these effects and help children to start school on a more equal footing with their more advantaged peers (Sylva *et al.*, 2004).

It was also found that, controlling for background factors, the types of pre-school that are most effective at increasing intellectual and social outcomes, are nursery schools and classes and integrated centres – where there is care and education combined. While high quality can be found in all settings and types of provider, it was found that nursery schools and classes had the highest scores in terms of pre-school quality, while private day nurseries, playgroups and local authority centres had lower scores. This is important as there was a significant relationship between quality of the centre and improved outcomes for children (Sylva *et al.*, 2004).

Importantly, the time frame over which the EPPE study has been carried out fits almost exactly with that of data that I have constructed. Moreover, the EPPE study has subsequently looked at the performance of children at the end of Key Stage 1, when the children are 7 years old. The follow up at age 7 was designed to assess whether the effects of pre-school on development, evidenced at school entry, persist further into schooling such that there is a significant influence of pre-school experience on performance in national assessments at the end of Key Stage 1 – after the children have spent at least two full years in primary school (see Table 2.1).

Year Group	Reception	1	2	3	4	5	6	7	8	9	10	11
Age of pupils at end of year	5	6	7	8	9	10	11	12	13	14	15	16
Key Stage	Foundation Stage	KS 1		KS 2				KS 3			KS 4	

Table 2.1: Key Stages of Compulsory Education in England

The EPPE results show that attending a pre-school centre compared with none, continues to be associated with significantly higher attainment levels in reading and mathematics, controlling for child, family and home learning environment effects (Sammons *et al.*, 2004) – so the 'home' group have not caught up with their peers who did attend pre-school. The strongest relationship is with reading attainment: attending pre-school is associated with an approximate one-quarter of a standard deviation increase in decimalised reading score[8]. For maths the positive effect is approximately one-sixth of a standard deviation. Thus the effect that is clear upon school entry has not 'washed out' by the end of KS1. While it has been reduced somewhat, there remains a significant attainment gap between those that do and those that do not attend pre-school after controlling for relevant background factors. It is not possible to fully disentangle the effects of duration, quality and effectiveness of pre-school attended in comparison with the 'home' group; however the findings show that these factors remain predictors of better cognitive attainment at KS1. *Within* the group of children who did attend pre-school, there is no longer a significant effect of longer duration or higher quality on outcomes at the end of KS1, however measures of the effectiveness of a pre-school centre at promoting cognitive progress continued to show a significant positive impact on attainments at KS1, which suggests that while the greatest differences are between those that had pre-school experience and those that did not, the nursery school and integrated settings continue to have the greater positive effect. Importantly, it is found that while the overall effect is similar across all socio-economic groups, there is a particular benefit of pre-school for disadvantaged children in that it raises them above the minimum expected levels at KS1, which means that on average, these disadvantaged children begin KS2 at a level that allows them to access the KS2 curriculum (Sammons *et al.*, 2004).

The government should be concerned about results at KS1 due to the links between early school performance and later scholastic and labour market attainments as witnessed in many of the US and UK studies cited above. Moreover, Currie and Thomas (1999) cite cogent evidence that cognitive outcomes at age 7 are a strong predictor of a range of later outcomes including school qualifications, employment and earnings. In the UK, Feinstein (2000) demonstrates the

[8]This is calculated as: the assessment level plus (the child's raw test score − the lowest raw score possible in that level)/(the highest raw score possible in that level).

predictive power of early attainment for later academic achievement. Looking specifically at KS1 results, Sammons *et al.* (1995) show that performance at this level is a good predictor of success at GCSE, the key high school qualifications that play a large role in determining the post-compulsory schooling destination of young people in the UK.

My contribution is to estimate the effect of the introduction of free early education places for all 3-year olds, at the aggregate level of the Local Education Authority – as the policy was implemented at the level of the LEA, it makes sense to evaluate the policy effects at this level. Local Education Authorities are the bodies responsible for the local administration of state sector education in England, and so it is important to examine how changing the education provision in LEAs over time affects overall results within these areas. If universal provision of free early education from age 3 is as effective as is suggested by the literature, we should see lasting positive effects of the policy. As I have the KS1 results from 2001-2006 for each LEA and given that the policy was introduced at two time points, depending on the level of deprivation in the LEA, this provides an identification strategy for evaluating whether the policy has had an effect on KS1 results. Given the zero price of the early education places, and the evidence of the excess demand for childcare, I assume that demand for early education places is 100% and that differences in take-up of places are the results of differences in supply between LEAs[9]. Unlike the EPPE study which does not have an experiment or natural experiment, I exploit this policy of effectively increasing supply of early education places (by utilising the private sector) to estimate their causal effect.

Moreover, since for each LEA I have the take-up of places in both the maintained nursery and primary schools and in the 'other' sector – comprising private, voluntary and independent providers and a small number of maintained settings other than nursery and primary schools – and given that the policy worked through utilising 'other' sector provision of early education places, I can evaluate whether the increase in take-up in what is largely the private sector, has had any effect on subsequent results at KS1. As detailed above, these KS1 reading, writing and maths tests, taken at age 7, are the first standard national assessments that children take after starting school and thus provide a useful benchmark.

Furthermore, I can address the question of whether there was greater benefit for the LEAs deemed to be in greatest need of the provision i.e. the 'pathfinder' LEAs. However, allowing the policy effect to differ between these two groups of LEAs weakens the identification strategy

[9]This assumption of demand being 100% is made by other authors also, see Brewer *et al.*, 2004, p.165

as I can no longer fully exploit the difference in timing of the policy's introduction to identify the policy effect. When looking at the simplest model with just separate dummies for the introduction of the policy in the poorer and better off LEAs, the identification comes from the assumption of common year effects but separate policy effects that are constant within type of LEA over time. In the model allowing for different effects of the actual levels of take-up in the 'poorer' and 'better off' LEAs, the identification is based on the fact that this is a policy change, from zero places provided for free in the 'other' sector to a positive and increasing number being provided and taken up. As the places for 3-year olds are provided for free, the take-up of these places in each setting should not be driven by income which is correlated with characteristics of the children and also therefore with outcomes. To the extent that all budget constraints are affected, the effect of this policy change should provide estimates of the effect of free early education in each sector on results. Again it is necessary to assume common year effects so that the effects of differences in take-up variables can be identified.

Different effects depending on which setting provides an early education place has implications for the way in which the Government should provide early years education – whether through expansion of capacity in state maintained school settings or by continuing to fund provision through the 'other' sector settings.

The advantage of having area level data is that it has been possible for me to build a panel of LEA inputs and results, and look at repeated measures of the KS1 assessments for each LEA, controlling for the fixed characteristics of each LEA, and evaluate the effect of the altering early education inputs. Analysis at the level of each individual child does not permit a panel to be created which would allow the fixed characteristics of the child and his/her family to be controlled, rather relying on richness of controls to capture heterogeneity – which may or may not be deemed sufficient. Therefore it is an advantage to have LEA level data allowing a panel to be created to control for time-invariant unobserved factors and analyse the effects of this change in policy implemented at the LEA level. The data that I have also has the advantage of delineating clearly the places provided by maintained nursery and primary schools and those provided in other settings, allowing an explicit contrasting of these two provider types. Moreover, the data covers LEAs across the length and breadth of England over more than half a decade, which will help to add to the picture of early education and its effectiveness across England during this time.

2.4 Context and Policy Detail

2.4.1 Background to the Free Early Education Places Policy

The Government has a broad agenda aimed at breaking cycles of poverty and social exclusion, and a well-publicised target of cutting child poverty in half by 2010 and eradicating it within a generation[10]. A cornerstone of the Government's strategy is helping people to break free from reliance on benefits and move into work – particularly lone parents, with a target of 70% employment among lone parents. The strategy operates on several levels, one of which is motivated by the reality that access to affordable, high quality childcare – or rather lack of access – is a factor that can restrict employment opportunities, particularly for the poorest families, and especially single-parent families.

Chevalier and Viitanen (2001) suggest that a lack of childcare was blocking maternal employment in the UK in the mid-1990s[11] and estimate that supply was pretty inelastic with demand outstripping supply by more than 50%[12]. This implies that the provision of good quality, affordable childcare by the Government, would reduce this queue, allowing mothers to return to work. The Government has responded to this and one part of their national childcare strategy has been to seek to ensure that there is high quality, affordable childcare for all children aged 0-14 in every community. Moreover, given the innate substitutability between childcare and early education places, this provides an additional motivation – on top of the potential positive effects on children's attainments – for the Government to be involved in providing free early education places. This is even more so the case in the disadvantaged communities in which there had been a lower supply of childcare and nursery places due to the risks to private providers in setting up in these areas and, on the demand side, financial constraints on poor parents making them unable to purchase the optimal levels of childcare and/or nursery for their children. For this reason the Government has specifically targeted several other initiatives at the poorest 20% of wards – through Early Excellence Centres, Sure Start Local Programmes and the Neighbourhood Nurseries Initiative[13]. These initiatives, like the free early education place policy, work to

[10] This pledge was given by the then Prime Minister, Tony Blair, in March 1999.

[11] See also reports by the Daycare Trust into lack of childcare availability: "The childcare gap: Briefing Paper 1" London: Daycare Trust, 1997; "Delivering the National Childcare Strategy: Briefing Paper No.1" London: Daycare Trust, 1999.

[12] The First National Childcare Strategy (www.surestart.gov.uk/_doc/0-BB628F.doc) also makes reference to the unmet demand for childcare, stating that 4 out of 5 non-working mothers would work if they could access the childcare of their choice (p. 14).

[13] 29 Early Excellence Centres were piloted in 1997 with 2/3 of them in the poorest 20% of population wards; subsequently rolled out to a total of 107 centres. Sure Start Local Programmes were introduced in 1999 in the poorest 20% of population wards, by 2002 there were 260 and by 2004 there were 524. The Neighbourhood Nurseries Initiative introduced in 2001 aimed to provide quality childcare places in the 20% most deprived wards

increase the supply of care and early education in the poorest areas. The 'other' sector places in the very poorest wards will include these Government maintained places, so we cannot interpret the 'other' sector purely as the private sector for the very poorest areas, however as an LEA comprises many population wards (on average 23), even in LEAs containing some of the poorest wards, there will be private providers. As well as these supply side interventions, there has been an increase in financial assistance to pay for childcare, via the childcare tax credit[14]. These initiatives, including the expansion of early education places, aim to improve outcomes for children and their parents.

Clearly there are broader benefits accruing to society when mothers are able to return to work, thus in evaluating whether the expansion of free early education places has brought positive benefits, it is acknowledged that my focus is narrowly on the effects on the children's results only. Even in the absence of positive effects of the policy on child attainments, there would remain a case for the Government to fund early education based on these maternal employment effects and the associated benefits.

2.4.2 Historical Context

Before 1997, the law did not oblige LEAs to make educational provision for children under compulsory school age, and the decision of whether to provide free nursery education, and if so how much to provide, was left to each individual LEA – leading to substantial variation in provision. According to Brewer *et al.* (2005) across the country provision ranged from zero free pre-compulsory education places provided, to a high of 27.5 places per 100 children in 1986. This variation in access to nursery education according to postcode and income persisted into the 1990s.

The Conservative government introduced a nursery voucher scheme in April 1997, which entitled all parents of 4-year olds to a voucher worth £1,100 to purchase nursery provision for their child from either a maintained, private or voluntary organisation, in an attempt to create a market for nursery education – allowing parents to choose the best place to send their child and in theory raising quality through competitive forces.

In May 1997 the new Labour government were elected and whilst initially they continued

in England.

[14] Initially (April 1997) there was the income disregard for means-tested family credit for families using formal childcare, this was replaced by the childcare tax credit introduced when the working families' tax credit replaced family credit (October 1999), this childcare tax credit was itself replaced by the childcare element of the working tax credit (April 2003 to present).

the roll-out of the piloted voucher scheme, they had a manifesto commitment to scrap the vouchers on the basis that they were costly and did not generate quality nursery places[15]. The nursery education voucher scheme was replaced by a local structure of Early Years Development and Childcare Partnerships and Plans[16], detailing how each LEA would provide sufficient early education places for children of the prescribed age within their area, whether this was supplied by the LEA itself (i.e. maintained places) or not[17]. Initially the prescribed age was 4 years old but there were plans already to extend the scheme to include 3-year olds.

The first National Childcare Strategy was launched in May 1998, part of which involved the immediate mandatory introduction of free part-time early education places for all 4-year olds whose parents request one[18] and also established that this would be extended to cover all 3-year olds, phased in according to the level of deprivation of the LEA. The 65 most deprived LEAs in England would extend the free early education places to 3-year olds in 1999-2000, while for the remaining 85 LEAs the extension to 3-year olds would not occur until 2000-2001[19]. Prior to 1999, any LEA could provide free early education places for 3-year olds as long as they had capacity in their maintained nursery and primary schools. However, in order to meet the commitment of places for **all** 3-year olds whose parents wanted a place, the private sector needed to be utilised. To pay for nursery education places supplied by providers other than the LEA itself, the Nursery Education Grant (NEG) was already available for places for 4-year olds from 1998. In tandem with the roll out of free places for 3-year olds, from 1999-2000 the NEG was available for 3-year olds' places in the 65 poorest LEAs and in the following year in the remaining 85 LEAs. Originally the target was to have a free place available for all 3-year olds by September 2004, though this was actually achieved by April 2004. When introduced in 1998-1999 the NEG was a maximum per child per year of £1,100. This rose each year so that it was £1,221 per child per year for 2002-2003[20].

For the period of my data, the funding for free early education places was paid directly to providers (maintained and non-maintained) by central Government[21]. Therefore, it is not the case that there is a potential trade-off between spending on under-5s and spending on over-5s

[15] The Labour Party Manifesto, 1997. The alleged costliness was due to vouchers being given to the parents directly which entailed high administration costs.

[16] Early Years Development and Childcare Partnerships and Plans were originally (1998) just Early Years Development Partnerships and Plans but were renamed to include Childcare in April 1999.

[17] This was established in law by the School Standards and Framework Act, 1998 (August).

[18] It was legal requirement officially from April 1999 as part of the School Standards and Framework Act, 1998.

[19] See the National Childcare Strategy: www.surestart.gov.uk/_doc/0-BB628F.doc.

[20] The NEG in the other years has been: 1999-2000, £1,160; 2000-2001, £1,188; 2001-2002, £1,218.

[21] Since April 2003 this has changed such that early education funding has been provided to LEAs by central Government via the formula spending share.

within the LEA – LEAs could not choose to focus funds on one at the expense of the other, thus there should not be a problem of the estimated effect of increased early education provision being mitigated by reduced primary spending.

There is currently a pilot scheme looking at extending the free early education places to cover 12,000 two-year olds living in the most disadvantaged communities[22] – hence it is pertinent to ask whether the extension of provision to 3-year olds in having a measurable effect on test results.

2.4.3 What is a Free Early Education Place and Where Can One Be Taken?

A free early education place consists of five sessions of early education per week, each session lasting a minimum of two-and-a-half hours (i.e. a morning or an afternoon), for 33 weeks of the year. All settings providing free early education places must be working towards Early Learning Goals and other features of the Foundation Stage Curriculum – which is the first stage of the National Curriculum focusing on the needs of 3- to 5-year olds and implemented primarily through planned play activities designed to develop children's emotional, physical, social and intellectual capabilities. Places are usually delivered in three 11-week terms – based on the terms of nursery schools and classes in the maintained sector[23].

Maintained sector providers of free early education places are funded and run by the local authority and are either: nursery schools and nursery classes, infant classes in primary schools, day nurseries, Children's Centres/Family Centres or other local authority providers such as Special Schools. Within the maintained sector, the vast majority of free early education places are provided in nursery schools and nursery or infant classes of primary schools, and children are not usually admitted before their third birthday.

In the non-maintained sector, early education places are provided by voluntary, private or independent bodies or by groups of parents, in a variety of settings such as non-maintained nursery schools, private nurseries, playgroups, childminders, or nursery classes attached to independent schools. These non-maintained sector providers must be registered with the LEA and named as partner in the Early Years Development and Childcare Partnership.

In order to fulfil this requirement, each setting must meet 14 national standards verified by an inspection by the Office for Standards in Education (Ofsted). The national standards specify

[22] Detailed in the Government Spending Review, 2004.

[23] From 2006 this has been extended to provision for 38 weeks of the year, in three terms of between 11 and 14 weeks. In its 10-year strategy released with the pre-Budget Report 2004, the Government sets out a plan to extend the free early education place provision to 15 hours per week by 2010, with the eventual goal of 20 hours per week.

that childminders can look after no more than 6 children under the age of 8; no more than 3 of which can be under the age of 5, and of these 3 no more than 1 may be under 1 year old. Private nurseries and playgroups must have a manager with a specified level of qualifications and number of years experience, as must all of the employees of the facility. Moreover a group can never exceed 26 children and there must be at least two adults on duty at any time. The staff-child ratio that must be adhered to for children in the pre-school age bracket is a minimum of 1:8. Maintained Children's Centres and Family Centres also have to meet these national standards.

Maintained nursery and primary schools are also inspected by Ofsted and required to meet certain recommended standards, ensuring that there is a minimum of 2 members of staff for every 26 children in a nursery class in a maintained nursery or primary school[24]. One member of staff must be a qualified teacher and the other a qualified nursery assistant. These requirements are also imposed on private nursery schools and nursery classes attached to independent schools.

So while all settings aim to deliver the same Foundation Stage Curriculum, they essentially represent different technologies for this delivery. Pre-school education had been provided by non-nursery/primary school settings prior to the introduction of the free early education place policy, however it is only since the advent of this policy in 1999-2000/2000-2001 that these early education places with their structured and regulated form and their own curriculum and teaching requirements, have existed outside the maintained schools system.

It is worth noting that at the start of the period (1998), while nursery schools and nursery/infant classes in primary schools were already being inspected by Ofsted, the 'other' settings – largely the childcare as opposed to formal education settings – had their quality regulated by a decentralised system, where it was essentially left to each LEA to ensure the quality of its childcare providers. (It was partly because of this rather fragmented system that Ofsted were given the responsibility to ensure the quality of childcare providers in September 2001[25]). Given that Ofsted were already inspecting nursery and primary schools, it is unsurprising that the EPPE study has found that the 'process' quality is highest in the nursery schools and nursery classes, as opposed to the other childcare settings.

The potential differences in quality across settings provides further motivation for the question I am asking. Has the increased NEG payment from the Government, allowing 3-year olds

[24] The minimum is 2 members of staff for every 20 children if the main teacher has other administrative duties (i.e. is also the head teacher).

[25] See Moore, L. (2000) "Who's Looking After the Children? Inspection of Registration and Inspection Arrangements for Under Eights Day Care Services." Social Services Inspectorate Report CI(2000)18.

to access early education in non-school settings, had any effect on children's results? Would a better strategy be to increase the capacity of maintained schools rather than effectively contracting out the commitment to provide early education? If the maintained nursery schools and classes are on average of better quality, it is relevant to ask whether the increased use of the private sector is having a negative impact on outcomes and does this depend on the deprivation level of the LEA?

2.5 The Model

2.5.1 Initial Policy Evaluation

In evaluating whether the policy of allowing free early education places for all 3-year olds has had an effect on results, the first basic model to look at is a simple linear panel regression model with fixed effects at the level of the LEA, and with just a simple [0,1] dummy to indicate the years in which the policy is in operation:

$$R^s_{jt+3} = \alpha_0 + \alpha_1 \text{policy}_{jt} + \mathbf{T}'\varphi + c_j + \epsilon_{jt+3} \tag{2.1}$$

where R^s_{jt+3} are the results outcomes: the percentage of children in LEA j at time *t+3* who attain a specified level in subject s, where $s \in \{$*reading, writing, maths*$\}$. The levels that I look at are L2B or higher, which is the level that children are expected to attain at KS1, and L3 or higher which represents very high achievement at KS1;

policy_{jt} is the [0,1] dummy to indicate that the policy is in operation in LEA j at time t;

$\mathbf{T}$ is a vector of year dummies; c_j is the fixed effect for LEA j; and ϵ_{jt+3} is the idiosyncratic error term for LEA j at time *t+3*.

It is necessary to estimate the model using the fixed effects estimator due to the nature of selection into the group of LEAs that have the policy implemented in the first phase. As outlined above, it was the 'poorer' LEAs, the 65 deemed to be in greatest deprivation, that were given the Nursery Education Grant to pay for early education places for all 3-year olds in 1999-2000, while the 'better off' LEAs did not receive the funding until 2000-2001. These first 65 LEAs to receive the policy funding are the 'pathfinder' LEAs, with the remaining 85 LEAs the 'non-pathfinder' LEAs. As well as being the first to have the policy implemented, these 'poorer' LEAs also have a results distribution which is lower than the distribution for the 'better off' LEAs. Consequently, in a cross sectional estimate of the effect of policy on results, we will pick up some of the negative effect of being a poorer LEA through the coefficient on the policy dummy – there is a correlation between the fixed unobserved component of the error term and the policy dummy, biasing the coefficient downwards.

Implementing a fixed effects regression controls for all of the time invariant characteristics of the LEA, treating the unobserved component of the error term for each LEA as a parameter to be estimated and therefore allowing a clean estimate of the policy effect. So the selection issue is dealt with since selection into the early treatment group is on the basis of fixed unobservable characteristics that are subsumed in the fixed effect and thus controlled for. The policy effect is identified through differences between LEAs in their within variation in policy status and results.

Included in the model are a set of year dummies with the first results year the omitted comparison year in each case. It is necessary to include year dummies to take account of any common trends in results due to the year of the test – though the assessments are standard across the country, there may be countrywide cohort effects or marking leniency changes that equally effect all LEAs in the different years. As the policy is implemented at a different time for two different groups of LEAs there is not a problem of the year dummies and the policy dummy being collinear. It is necessary to assume that the year effects are common across the two groups of LEAs in order for this identification strategy to be successful. However I believe that this is a reasonable assumption given that there should be no reason why the year effects would not be common to all LEAs if they are driven by variations in marking standards, since the assessments are marked to an externally implemented national criteria that is standard across the country. Moreover there is no reason *a priori* to assume that there are differing cohort effects depending on whether an LEA is in the pathfinder group or not.

As I also wish to look at the extent to which the policy has affected the areas of most concern i.e. the LEAs that were deemed most in need of the policy and therefore had the policy implemented first, I also estimate the policy evaluation regressions allowing for a different policy effect depending on whether the LEAs were the poorer 'pathfinder' or the better off 'non-pathfinder' LEAs:

$$R^s_{jt+3} = \gamma_0 + \gamma_1 \text{policy}_{jt} * \text{pf}_j + \xi_1 \text{policy}_{jt} * (1\text{-pf}_j) + \mathbf{T}'\phi + c_j + \epsilon_{jt+3} \tag{2.2}$$

where all variables are as per their definitions above, with pf_j a [0,1] dummy for the path-finder LEAs. Thus the coefficient γ_1 is the policy effect for the poorer LEAs and ξ_1 is the policy effect for the better off LEAs. Clearly this weakens the identification strategy as I can no longer fully exploit the difference in timing of the policy's introduction between the two groups of LEAs. The assumption of common year effects means however that I can identify separate policy effects for the two groups of LEAs without them being collinear with the year dummies.

Whether estimated separately or in single policy dummy variable, it is also necessary to assume that the policy effect is an intercept shift that is constant in each year that the policy is in operation – otherwise it is clear that some of the policy effect in the later years, when all LEAs have the policy in operation, could be subsumed in the year dummies thus we could not delineate the separate policy effect were we not to assume it to be constant and identified through the years when the policy dummies are not equal to each other.

2.5.2 A more detailed look at the policy implementation

In addition to looking at the basic model assessing the policy effect by looking at results before and after the policy implementation, I now want to move on to implement a more sophisticated approach. While the policy itself specifies that all 3-year old children are entitled to a free early education place that the LEA must provide either in a maintained school setting, another maintained setting, or fund with another provider, it is the case that 3-year olds could attend maintained school settings and receive an early education place prior to the policy introduction, if the LEAs had the capacity. Therefore it is necessary to control for the take-up of places in the maintained nursery and primary schools both before and after the policy implementation.

As discussed above, the expansion of free early education places for 3-year olds, since 1999-2000, has essentially been in the private sector as 4-year olds account for much of the maintained sector capacity. However, all children are entitled to an early education place when they are 4 years old also. Since the introduction of the policy for 3-year olds was a shock to the whole pre-compulsory education market – for 4-year olds as well as 3-year olds – the effects may work through a number of avenues which need to be considered in the model. Therefore rather than looking at what happens when these children are 3-year olds in isolation, I look at what is happening at each age, within the same structure: I include the take-up rate of early education places by 3-year olds in an LEA and the take-up rate of early education places by 4-year olds in the same LEA the following year i.e. when these same 3-year olds are 4 years old. The effects of changes in these take-up rates on results in KS1 tests two years later – when these children are assessed at the end of Year 2 – is estimated.

The main question that the model asks is: controlling for the take-up in each sector of free early education by 4-year olds, what is the effect on KS1 assessments, of introducing free early education places for these children when they were 3 years old?

The model is again a linear panel regression with fixed effects at the level of the LEA:

$$R^s_{jt+3} = \pi_0 + \pi_1 \mathrm{S3}_{jt} + \pi_2 \mathrm{O3}_{jt} + \pi_3 \mathrm{S4}_{jt+1} + \pi_4 \mathrm{O4}_{jt+1} + \mathbf{CC}'_{jt}\delta + \mathbf{X}'_{jt+3}\zeta + \mathbf{T}'\phi + c_j + \epsilon_{jt+3} \quad (2.3)$$

again R^s_{jt+3} are the results outcomes: the percentage of children in LEA j at time $t{+}3$ who attain a specified level (2B or 3) or higher in subject s, where $s \in \{reading,\ writing,\ maths\}$;

$\mathrm{S3}_{jt}$ is the percentage of the 3-year old population in LEA j at time t, taking a free early education place in a maintained nursery school or a nursery class in a maintained primary school;

$\mathrm{O3}_{jt}$ is the percentage of the 3-year old population in LEA j at time t, taking a free place with a private, voluntary or independent provider or in a non-school maintained setting[26];

$\mathrm{S4}_{jt+1}$ is the percentage of the 4-year old population in LEA j at time $t{+}1$, taking a free early education place in a maintained nursery school or nursery/infant/reception class in a maintained primary school;

$\mathrm{O4}_{jt+1}$is the percentage of the 4-year old population in LEA j at time $t{+}1$, taking a free place with a private, voluntary or independent provider or in a non-school maintained setting[27];

$\mathbf{CC}'_{jt}$ captures the availability of childcare places generally in the market in LEA j at time t i.e. at the time that the children are 3 years old and can redeem their free early education place. $\mathbf{CC}'_{jt}$ is $\begin{bmatrix} \mathrm{dnp}_{jt} & \mathrm{cmp}_{jt} & \mathrm{pgp}_{jt} \end{bmatrix}$ where dnp_{jt} is the places available in day nursery per 100 children aged 3 or 4 in LEA j at time t. cmp_{jt} and pgp_{jt} are similarly defined for childminder places and playgroup places respectively.

$\mathbf{X}_{jt+3}$ is a vector of the characteristics of schools in LEA j at time $t{+}3$, such as the average KS1 class size and the ethnic composition of schools. As before, $\mathbf{T}$ is a vector of year dummies; c_j is the fixed effect for LEA j; and ϵ_{jt+3} is the idiosyncratic error term for LEA j at time $t{+}3$.

As each of the take-up rate variables is defined as the proportion of the population of the relevant age in the LEA taking their free early education place in that setting, the omitted category are the children who do not take a free early education place. The interpretation of

[26] As outlined above, this 'other' sector includes a small number of maintained settings other than nursery schools/classes but it is essentially private, voluntary or independent provision.

[27] see footnote 26

these coefficients is therefore the effect of increasing the take-up rate in a particular setting (by increasing supply) compared to not taking up a free early education place in any setting.

We know from looking at the data itself and from previous research (see Brewer *et al.*, 2005; Melhuish, 2004) that LEAs have different characteristics – in terms of demographic composition, class and socio-economic status (SES) mix, employment, education. These differing characteristics have had an effect through time to alter the levels of provision of early education and childcare services within each LEA, so different areas start the panel with different initial endowments. Moreover, while it is possible to some extent to control for observable differences between the LEAs, there may be unobserved characteristics of the LEA that are correlated with both the take-up rate of early education places in each sector, and the results in KS1 assessments. For example, it may be that an LEA has a high proportion of high SES families and that these families put a strong emphasis on education. We would expect that take-up of early education places in this LEA would be relatively high – both before and after the policy introduction – and that independent of early education, KS1 results would also be higher given the parental attitude towards the education of their children. In a cross sectional estimate this would result in an upward bias in the estimated coefficient on early education place take-up.

As we know that the balance between take-up in public and private settings, and the take-up rates themselves, differ between LEAs according to observed characteristics, and we expect that this is also the case for unobserved characteristics such as attitudes and tastes, we cannot make inferences of causal effects from cross-sectional estimates because of the bias resulting from these confounding unobserved characteristics. Estimates using the between estimator, find that having high levels of childcare provision – particularly playgroups and childminders – and lower levels of maintained sector place take-up is associated with higher assessment results. However it is clear that this is picking up the correlation between unobserved characteristics of the LEAs (such as socio-economic status mix) that influence both the amount of childcare places available, the take-up in the maintained sector and also the KS1 results. In addition, the between estimates show that having high levels of take-up by 3-year olds in the 'other' sector is associated with lower results, though again as we know that the time-average take-up in the 'other' sector is higher for the poorer LEAs because they have the funding for these places before the better off LEAs, the significant negative effect on results is capturing the correlation between the unobserved characteristics that lead to this higher average take-up and also lead to lower average KS1 results. For details of the between estimator results see Appendix A.7.

Implementing the model as a fixed effects regression means that we lose the information from the cross-sectional variation in take-up; however, Hausman tests on the correlation between the fixed unobserved component of the error term and the regressors provide evidence supporting the prior that there is correlation between these time-invariant unobserved characteristics of the LEA and the regressors (for details of the Hausman test results see Appendix A.7).

By implementing the model as a fixed effects regression, we circumvent this problem of bias owing to the correlation between the fixed unobserved factors – that we suspect are correlated with the outcomes – and the regressors in the model. All of the time-invariant characteristics of the LEA that may cause a bias in estimates, plus factors such as the size of and number of schools and the quality of teaching will be subsumed in the fixed effect – to the extent that these factors are constant throughout the time-span of the panel. Given that the panel is quite short – spanning 6 years – it is reasonable to assume that the class/SES mix of the LEA remains stable over the time of the panel. Similarly, I do not believe it too heroic an assumption to make, that the average quality of teaching within the entire LEA's schools remains approximately constant throughout the time of the panel.

As detailed above, the fact that the policy was introduced at two different time points provides additional variation in the key explanatory variable – the take up rate of early education places in the 'other' sector. Moreover, key to the identification strategy is the fact that changes in take-up of early education places by 3-year olds in this sector are theoretically exogenous to income. While previously attempts to quantify the effect of, for example, private sector nursery attendance on outcomes was undermined by the differential use of these settings according to their costs, the fact that the policy change allows private places to be taken by any 3-year old child and paid for by the Government, means that the use of early education places in each setting is theoretically no longer affected by budget constraints. In terms of identification, the fixed effect deals with between LEA differences in take-up rates that are correlated with time-invariant unobserved characteristics which are also correlated with results. Within each LEA, the identifying assumption has to be that changes in take-up rates in each sector are exogenous – i.e. implying that they are random between the maintained school settings and the 'other' sector settings. As this may not be the case there is a selection issue: the coefficient on the take-up rate in each sector may be biased up or down by picking up the effect not of the setting itself but of the characteristics of the type of children that attend that setting. However, to the extent that the key selection mechanism is income, this selection issue is dealt with – there is no cost of these places in either type of setting and therefore there is no selection according to which families can afford to pay for private places. Arguably it may continue to be the case that, within an LEA, changes in the take-up in the two sectors continues to differ according to tastes that are correlated with income, nevertheless income itself as a direct confounding factor has been removed. Moreover, it is not clear how this selection would work – would better off parents within an LEA send their children to private settings or, would they be more likely to use the maintained school setting, in the knowledge that they are inspected and rated highly as early education providers? Similarly would poorer parents look to use the private settings that were previously unavailable to them or choose maintained school settings? Therefore I believe it reasonable to work on the assumption that changes in each sector within an LEA are exogenous – that the shock of the policy introduction did not lead to differential take-up across the sectors according to characteristics that are also correlated with results, and therefore I can obtain unbiased estimates of the effect of changes in take-up in each sector on KS1 results.

The fixed effect estimated for each LEA accounts for time-invariant heterogeneity at the LEA level, and as before, the time-dummies included in the model will account for any common time variation that affects results. There may still be time-varying heterogeneity at the level of the LEA that could bias estimates. However, it is difficult to imagine what this time-varying unobserved heterogeneity could be. Almost by definition, anything that is a distinct characteristic of an LEA must be reasonably time-invariant in order to be a recognised feature of the LEA - as opposed to some random variation. Therefore the LEA level fixed effects included in the estimation should do a good job of accounting for differences between the LEAs, and allow me to estimate the effect that 'other' sector free early education places have on outcomes, purged of the effects of heterogeneity between the LEAs that is correlated with the take-up rates and the outcomes.

There is a problem with the identification of the effect of the increased 'other' sector take-up by 3-year olds, in that, as the initial take-up is zero in all LEAs but increasing year-by-year as the policy is implemented, this inevitably reduces the variation between LEAs in terms of their within variation in this variable. However, the fact that the policy was introduced at two different time points adds to the variation in this variable and strengthens the identification strategy.

Furthermore, there is also inevitably a strong correlation between this take-up rate and the year dummies. However, since we have the take-up rate of 'other' sector places in this more complex model, as opposed to just [0,1] policy dummy, means that there is more local variation to exploit for identification, over and above just the switching on of a year dummy.

The error term for each observation, u_{jt+3}, is comprised of c_j and ϵ_{jt+3}. The fixed effect, c_j, contains the non-time varying characteristics of the LEA, therefore implementing a fixed effects regression will reduce the correlation between the error terms of the LEAs as it controls for the presence of the c_j. However there may still be some correlation in the errors of observations from the same LEA (i.e. some correlation between the ϵ_{jt+3} parts of the error for observations from the same LEA) thus clustering at the level of the LEA is necessary. The clustering allows the idiosyncratic errors for an LEA to be correlated over time but the idiosyncratic errors across LEAs are assumed independent. Moreover, clustering automatically provides the robust standard errors that allow for heteroskedasticity in the error terms. Importantly, the estimates of the standard errors do not assume a specific functional form for the within LEA correlation or heteroskedasticity.

Given that the LEAs first to receive the NEG for 3-year olds have higher levels of deprivation and lower educational attainment outcomes, we would expect – and the Government hopes – that increasing take-up of free early education places in the poorer LEAs would have a greater effect than in the more prosperous areas. In the second specification of this more complex model, I am again interested in looking at whether this proves to be the case – whether there are differing effects dependent on whether or not the LEA is one of the 65 the Government identified as having higher levels of deprivation. Therefore, I re-estimate the following specification of the model:

$$\begin{aligned} R^s_{jt+3} = \beta_0 &+ \beta_1 \mathrm{S3}_{jt} * (\text{1-pf}_j) + \lambda_1 \mathrm{S3}_{jt} * \mathrm{pf}_j + \beta_2 \mathrm{O3}_{jt} * (\text{1-pf}_j) + \lambda_2 \mathrm{O3}_{jt} * \mathrm{pf}_j \\ &+ \beta_3 \mathrm{S4}_{jt+1} * (\text{1-pf}_j) + \lambda_3 \mathrm{S4}_{jt+1} * \mathrm{pf}_j + \beta_4 \mathrm{O4}_{jt+1} * (\text{1-pf}_j) + \lambda_4 \mathrm{O4}_{jt+1} * \mathrm{pf}_j \\ &+ \mathbf{CC}'_{jt}\delta + \mathbf{X}'_{jt+3}\zeta + \mathbf{Z}'_{jt}\theta + \mathbf{T}'\phi + c_j + \epsilon_{jt+3} \end{aligned} \tag{2.4}$$

All variables are as defined above, and in addition $\mathbf{Z}_{jt}$ is a vector of characteristics of LEA j at time t, capturing the deprivation level of the LEA at the time that the children were 3 years old – elements include the male economic inactivity rate, the male average weekly pay rate, and the manufacturing jobs rate.

The other difference from the earlier specification is that the slope coefficients on the take-up rate of places in each sector and at each age, are allowed to differ according to whether the LEA was one of the 'poorer' pathfinder LEAs who were first to receive the NEG or not. The β coefficients refer to the effects of take-up for the 'better-off' LEAs, while the corresponding λ coefficients capture the effects for the pathfinder LEAs. Clearly the allocation into these two separate groups was not random, it was on the basis of the characteristics (deprivation level) of the LEAs, which may be varying to some extent over time. Therefore it is important that I control for this as far as possible in the model – or else the estimated coefficients could be biased through picking up effects of being (non-)deprived on the KS1 results. The Government produces indices of multiple deprivation which consider a range of indicators of disadvantage. However, these indices are constructed only periodically, and during the time-range of my data there is only one available index of multiple deprivation constructed for the year 2000. Obviously if this time-invariant characteristic was included it would immediately be subsumed into the fixed effect.

The fact of being one of the poorest LEAs in 1998 – when the decision was made over which LEAs would receive the funding first – is a fixed characteristic and therefore part of the fixed effect and thus not a problem. That the groups of LEAs are different in terms of deprivation would not bias their coefficients as long as deprivation is just a fixed characteristic of the LEA that does not change during the time period of the panel. However as this is not the case, deprivation has different dimensions that are variable, this may introduce a bias into the separately estimated coefficients. It could be that in the poor LEAs certain characteristics change over time in a different way to the changes in these characteristics in the more prosperous LEAs. If this is the case, then we would worry that it is the changes in these other variables that are influencing the outcomes and therefore would be biasing the coefficient on the 'other' sector take-up rate which we know ***is*** changing differentially according to the deprivation level of the LEA. Therefore in order to control for characteristics correlated with deprivation, which determined allocation to the pathfinder group, I include in the vector $\mathbf{Z}_{jt}$ time-varying features of the LEA that can indicate levels of disadvantage. This should prevent the coefficients on the take-up rate variables in each group being biased by the influence of these other time-varying factors correlated with deprivation level.

As outlined above, the identification strategy is weakened when looking at separate effects of changing take-up of places by 3-year olds in the 'other' sector, since I can no longer exploit the variation owing to the pathfinder group of poorer LEAs having the policy implemented a year before the remaining LEAs. However, the change in policy itself – from zero places in the 'other' settings being funded to a positive and increasing amount being funded by the Government – is still a change that is exogenous to parents' income in each group of LEAs and should therefore be able to provide identification of the causal effect of these free early education places on later outcomes. I continue to assume that any year effects on results are common across all LEAs, and therefore the separate effect of take-up in each of the different groups can be identified as distinct from the year dummy effects. Again the fact that there is more local variation in the take-up variables than in the year dummies improves the identification.

It is noted that while I can answer the question of whether the increased funding for early education places for 3-year olds had any positive (or negative) effects on children's KS1 performances, there are a number of things that I am unable to answer. For example, it is not possible to distinguish between a zero effect, and an effect that is 'washed out' by the end of Year 2. However, in some respects this does not matter in that, if there is an initial effect but it washes out after two years of schooling, then there is really no effect – no lasting effect in any case, and it is effects that last into middle-childhood and beyond that are of concern.

In assessing the success of the policy, I am focusing narrowly on any effects on KS1 results, however, there are other positive effects of early education on social, behavioural and emotional outcomes that are not captured in KS1 results[28] which are increasingly being recognised as of great importance in terms of scholastic and later success in the labour market and society in general (see Carneiro and Heckman, 2003; Melhuish, 2004; Waldfogel, 2007). Moreover, as mentioned above, there are potentially very large benefits, in terms of child outcomes, parental outcomes and societal outcomes, if the policy facilitated the return to work of parents, particularly lone-parents. Thus I am not attempting to formally quantify the costs and benefits of the policy, I am focused narrowly on the effectiveness of this policy in terms of improving early educational attainment as measured by Key Stage 1 results.

2.6 Data

2.6.1 Data Sources

The dataset that I use to estimate the model has been constructed using data from a number of sources. The primary sources of data are the Department for Education and Skills (DfES) who

[28] To the extent that behavioural improvements facilitate learning there will be some capture of these effects though not explicitly.

provide information on the number of free early education places taken, by age, for each sector in each LEA in January each year; and National Statistics who provide data on the population, by age, in each LEA in January each year. Using these two sources I have constructed the take-up rates of free early education places separately for 3-year olds and 4-year olds in each LEA in January each year (for details of the variables construction see the Data Appendix (Appendix A.1)).

The first year of data is for children who were 3-years old in January 1998, and I have the information annually until January 2003 – creating a panel that spans 6 years.

The DfES also provides the information on the number of childcare places available in the different childcare settings – day nursery, playgroups and childminders[29] – in each LEA in each year[30]. As I am interested primarily in the effect of the expansion of funded places for 3-year olds, I am interested in the childcare market in each LEA at the time that children are 3 years old. However, since there are 4-year old children in this market also demanding childcare places, I estimate the number of places available relative to the size of the population aged 3 **or** 4 at the time the cohort in question are aged 3 – in recognition of the fact that these childcare places are not just for 3-year olds (again, see the Data Appendix (A.1) for details).

The responsibility for collection of this childcare information was with the Department of Health in 1998 but transferred to the DfES for 1999-2001 before being transferred to Ofsted in September 2001. However, when Ofsted took on the responsibility of collecting the information on capacity, they were also charged with the responsibility to inspect the quality of childcare providers, and this transition resulted in the failure to collate the data on childcare place provision in 2002. Therefore 2002 is missing from the data, putting a one-year hole in my 6-year panel, leaving 5 years of usable data: 1998-2001 plus 2003.

The DfES also provides information on the characteristics of maintained schools in each LEA[31], allowing controls to be added for the effects of average class size for KS1 classes, and the ethnicity of pupils in maintained primary schools in the LEA – which I use to construct the percentage of non-white children in each LEA's maintained primary schools.

[29] In 2003 the childcare data categories change to be full-day care and sessional day-care (in addition to childminders) - however the definitions of full-day care and sessional day-care are such that they continue to be day nursery and playgroup respectively. The only substantive change is that the full day care category includes places in family centres which were previously a separate category.

[30] The DfES childcare data is for March each year, thus is slightly after the free early education place data, though it is fair to assume that the availability of childcare places in March is a good approximation to the position in January.

[31] Available at www.dfes.gov.uk/rsgateway/DB/VOL/index.shtml in the annual Statistics of Education: Schools in England, LEA level included publications.

Using data from the Labour Force Survey (LFS)[32] I construct variables to attempt to control for the level of deprivation in the LEA. Using the LFS local area data for the relevant quarter, I construct for each LEA various measures of the local economic structure and prosperity such as the economic inactivity amongst working age males, the unemployment rate amongst working age males, the unemployment rate and economic inactivity rate for 16-24-year olds, the professional occupations rate, and the manufacturing occupations rate.

From the Annual Survey of Hours and Earnings[33] I also construct the following mean and median pay measures for full-time male workers: gross weekly pay, weekly pay excluding overtime, weekly pay basic, gross hourly pay, and hourly pay excluding overtime, for each LEA in each year that I look at data for 3-year olds.

All of the independent variables data is annual for the years 1998 to 2003, though as mentioned, the missing childcare data in 2002 makes it a 5-year rather than 6-year panel.

The dependent variables data also comes from the DfES, who publish the results of national Key Stage 1 standard assessments[34]. For each of the results years 2001-2006, the DfES publish the percentage of children in each LEA's maintained schools attaining various levels in their KS1 assessments of reading, writing and mathematics. As mentioned above, the outcome variables that I am interested in are the percentage of children attaining the expected level, 2B or higher, and the percentage of children attaining level 3 or higher. This allows me to look at different parts of the distribution of results and examine whether early education is more effective in raising children to the expected standard or raising them to a level above this standard, and whether this depends on the deprivation level of the LEA. The results at level 3 or higher are only available from the results year 2002 onwards, thus the analysis at this level relies on four years of data for each LEA (results years 2002, 2003, 2004 and 2006) rather than five.

For the years 2001 to 2004, the published results refer to the children's attainments in a standard national task/test, thus this is a consistent dependent variable. From 2005 onwards however, the assessment altered slightly to be a teacher assessed level for the child – based on their performance in the standard national task/test but also taking into account the teacher's own knowledge of the child. Clearly this is something that could potentially affect results, and could affect things differentially across LEAs depending on the teachers' attitudes within each

[32] Provided by the Data Archive, under project number: 18375.

[33] Available from National Statistics via the NOMIS official labour market statistics website www.nomisweb.co.uk.

[34] Available at //www.dfes.gov.uk/rsgateway/DB/SFR/index.shtml in the National Curriculum Assessments of 7 year olds by Local Education Authority publications for each year.

LEA[35]. In light of the potential problems caused by this alteration in assessment method, I robustness check any results by running the regressions both with and without 2006 results data included (see Appendix A.6).

Clearly there is a time-lag between the time that the children access their free early education place, and the end of Year 2 in primary school, when they have their KS1 assessments. This means that in terms of results data, the panel runs from 2001 to 2006, with a missing year in 2005 owing to the missing childcare data in 2002. The link up of the data is as follows:

1998	**1999**	**2000**	**2001**	2002	**2003**	*t*	**Early education data when age 3 (Jan)**
↓	↓	↓	↓	↓	↓		
1999	**2000**	**2001**	**2002**	2003	**2004**	*t+1*	**Early education data when age 4 (Jan)**
↓	↓	↓	↓	↓	↓		
2001	**2002**	**2003**	**2004**	2005	**2006**	*t+3*	**KS1 SATs scores when age 6/7 (May)**

Table 2.2: Panel Structure

With regard to the policy, for the first two 'waves' of the panel, 3-year olds could have an early education place in a maintained nursery or primary school if the LEA had capacity and allowed 3-year olds to be admitted. By the time of wave three (January 2000), the Nursery Education Grant was available to the 'poorer' LEAs allowing early education places to be taken by 3-year olds in the private sector, and by wave four (January 2001) this was extended to all LEAs. By the time the first pseudo-cohort of 3-year olds were 4-years old i.e. January 1999, the NEG was available for all LEAs for 4-year olds and all were entitled to a free early education place.

The structure of the link-up immediately leads on to a number of data issues.

2.6.2 Data Issues

There is a measurement error issue inherent in the data, owing to the structure of schooling in England and the timing of the collection of the data on the number of children aged 3 and age 4 taking a free early education place. The law states that children attain compulsory school age when they turn 5 years old and must be in full-time schooling from the start of the academic term following their 5^{th} birthday. However, in reality all LEAs operate an admissions policy that sees children begin school either at the start of the academic term or at the start of the

[35] Anecdotal evidence has suggested that teachers are likely to deliberately mark children's levels down in the KS1 assessment, motivated by the prospect of a falsely inflated 'value-added' measure when the children are subsequently assessed at KS2.

academic year during which they will turn 5. Children start in a Reception or an infant class, and will not start Year 1 until the September when they have already reached age 5. Most will have turned 6 by the end of Year 1, and thus the majority of children will have turned 7 by the end of Year 2 when they take their KS1 assessment tests.

The measurement error arises because the school year-group that a child is in, is determined by their age at 31st August. However, the data on children taking free early education places records the children's age in January[36]. This means that children recorded as age 3 and taking a free early education place in January could be in one of two school years. For expositional purposes we can make the simplifying assumption that births are evenly spread throughout the year, in which case two-thirds of the children recorded as age 3 on 1st January (year t) will have turned 4 by August 31st of that year, and will therefore be starting Reception class in the September of that year (t). They will start Year 1 in the following September (year $t+1$), start Year 2 in the September after that (year $t+2$) and will sit their KS1 assessments in the May of the year following this (year $t+3$). The other one-third of children aged 3 in January will have only just turned 3 in the months from the previous September to December, and so by 31st August (year t) will still be 3 and therefore not starting Reception until the following September (year $t+1$) and not start Year 1 until the September after that (year $t+2$) and will not therefore take their KS1 assessments until year $t+4$.

LEAs operate different policies in terms of whether it is the start of the academic year that the child turns 5 or the start of the academic term that the child turns 5 that they are brought into Reception class, however, this does not affect when they will start Year 1. Those that start in the academic term rather than academic year that they turn 5, have potentially fewer terms in Reception class but will start Year 1 when they are age 5 – the number of terms in Reception class adjusts such that children are always age 5 when they start Year 1 (see the Data Appendix (A.1) for a practical example of how the school starts are determined by month of birth irrespective of the regime of the LEA).

Data from National Statistics[37] shows that in the years of my data (i.e. births in the calendar years 1994-1999) just over two-thirds of annual births in England and Wales were in the first eight months (this is the case for each year bar 1996 in which it was 66.04%)[38]. This implies

[36]Children are recorded in January according to their age on 31st December in the previous year.

[37]Birth Statistics, Review of the Registrar General on births and patterns of family building in England and Wales, 2003, National Statistics Series FM1 no. 32, 2004.

[38]Percentages born in the first 8 months: 1994. 67.35%; 1995, 67.18%; 1996, 66.04%; 1997, 67.28%; 1998, 66.85%; 1999, 67.02%.

that, at the aggregate level, for two-thirds of children the link up of 3 years between the year that they are observed as a 3-year old and the year that they take their KS1 assessment will be correct.

This measurement error creates a problem with the accuracy of the estimates of the effect of free early education places on results – because for each year, one-third of the pupils assessed were 3-year olds in the data four years earlier rather than three years earlier. Put another way, approximately one-third of the cohort that we measure as 3-year olds and link to the results data three years later do not in fact take their assessment until four years after we recorded them as 3-year olds.

The measurement error should not cause a systematic bias in the estimates since it results purely from the distribution of births throughout the calendar year – and as such is should not be correlated with the right-hand side variables. As the provision of free early education places for 3-year olds changes, this will not cause any change in the proportion of births in the months September to December. Moreover, I assume that within each LEA over time, the distribution of ability does not covary with the change in the provision of free early education places for 3-year olds, in which case there should not be a systematic bias introduced to the relevant coefficients on the right hand side. It may be the case that the size of the measurement error differs between LEAs, but as long as this is not correlated with the early education place take-up variables, there will not be a systematic bias introduced.

We know that the proportion of births in the first eight months of the year in England and Wales as a whole remains approximately constant (at two-thirds) throughout the time span of births for the children in my panel of data, whilst the provision of free early education places changes – therefore at the aggregate level there is no relationship between the measurement error and key explanatory variables. I make the assumption that this stability of the measurement error is a feature also exhibited at the level of the LEA, such that there is no systematic bias introduced.

Another issue in the data, concerning this time-lag between the free early education and the KS1 assessments, is the possibility of movement between LEAs in the interim. This again potentially creates a measurement error issue. However, again this should not introduce a systematic bias into coefficient estimates, only attenuate the estimate of the causal effect of free early education on KS1 outcomes. If movement between LEAs in the interim between having an early education place as a 3- or 4-year old and taking KS1 assessments aged 7, is correlated with changing provision of free early education places then this would bias estimates of the effect of free early education provision. However, there is no reason to believe that this would be the case – it is not obvious why increasing (decreasing) take-up of early education places in an LEA would lead to movement into or out that LEA after a child has taken a place but before he/she has completed the first couple of school years. This problem is not something that I can control for given the limitations of the data that I have, thus it is necessary that I assume that movements across LEA boundaries are not correlated with early education take-up. If this assumption is indeed valid then there should not be a systematic bias introduced into the coefficients.

In sum, these measurement error problems should lead not to a systematic bias in the coefficients but to an attenuation bias in the coefficient estimates. As there is certain measurement error in the data I must assume that there is an attenuation bias operating, therefore I consider the positive coefficients to be the lower bound of the estimate of the effect (and any negative coefficients to be the upper bound of the estimate of the effect).

There is an issue concerning the construction of the take-up rate variables themselves, owing to a difference in the measurement of take-up across the sectors. For attendance in the maintained nursery and primary schools, the number of children attending at least one funded session is recorded – i.e. at least one morning/afternoon per week. In the 'other' sector, the part-time equivalent number of places is recorded, such that a child attending for three sessions per week would be counted as 0.6 places. However, the data indicates that these measures are close to being equal, furthermore in evaluating take-up the DfES publications and other authors (see Brewer *et al.*, 2005) treat these take-up rates as comparable and I also make the assumption that they are comparable quantities (for further details of the reasoning behind this assumption see Appendix A.1).

Another small consideration in interpreting the data is the fact that free early education places can be redeemed in some childcare settings[39], which raises the possibility of double counting – we count children taking their free early education place in the 'other' sector with, for example, a childminder, and also count the provision of childminding places in the LEA. However, the childcare availability variables are included as an indication of the levels of provision of childcare in the local area, since this is a relevant factor in the early education market, as a substitute to nurseries and other more formal early education settings. At worst this correlation would lead to a reduction in the ability to identify the effect of private sector early education

[39] As long as the provider is a member of the Early Years and Childcare Development Partnership.

places as distinct from the effect of the availability of childcare places. Moreover, there is a non-overlap between the two sectors. Firstly take-up of places in the 'other' sector includes those taken in independent schools and other maintained settings that are not nursery or primary schools, and these are not covered by the childcare availability variables. Secondly the childcare availability variables cover all places available, regardless of who is paying for them – there will be children receiving their free early education places at the childcare providers and being paid for by the NEG, but there will also be many other places being taken and paid for by the parents.

Related to this, there may also be the possibility that children are recorded taking their free early education place and then go on to attend an independent school and thus do not have their results included when I estimate the effects on results in maintained schools. If children are recorded taking their early education place in the maintained sector but then go to a private or independent school (i.e. a non-maintained school) for their primary education then this will present a measurement error but it should not be an error that is correlated with changes in the explanatory variables – it is not clear why increased (decreased) take-up by 3-year olds of early education places in the maintained sector would lead to more parents choosing to send their child to a non-maintained primary school. Thus the measurement error should not be correlated with this explanatory variable.

However, there may be a problem of systematic measurement error, to the extent that children who attend an independent school for their free early education place stay in the independent sector for their primary education – which possibly could be the case. If this is the case, then increasing take-up by 3-year olds of free early education in the 'other' sector will be correlated with an increased measurement error.

Unfortunately data is not available at the LEA level, on the number of children taking free early education places at independent schools (they are counted as part of the 'other' sector but the independent schools contribution to this sector is not separately recorded). However, this information is available for England as a whole. From this data, it can be seen that in January 2000, the independent schools account for just 6% of the free early education places taken by 3-year olds in the 'other' sector, which itself accounted for 14.5% of free early education places taken by 3-year olds. Thus in terms of the total number of free early education places taken by 3-year olds, this is less than 1%[40]. The only other years in which the independent schools could

[40] It is 0.9%, based on figures calculated from "Provision for children under five years of age in England: January 2004 (Final)", National Statistics Statistical First Release for the DfES, SFR 39/2004, October 2004.

provide a free early education places for 3-year olds were 2001 and 2003[41].

In 2001 the independent schools accounted for 5% of free places for 3-year olds taken in the 'other' sector, though this sector had increased to be 35% of the free early education places taken by 3-year olds. This means that in terms of the total number of free early education places taken by 3-year olds, independent schools accounted for 1.7%.

By 2003, the independent schools were providing 6% of the 'other' sector free places taken by 3-year olds, but the sector had increased to account for 56% of the total number of free places taken by 3-year olds. However, this still means independent schools are only accounting for 3.2% of the total number of free early education places taken by 3-year olds.

There is clearly an increase in the use of independent schools in providing free early education places for 3-year olds – this is the Government's policy in action – and this may mean an increase in the use of independent schools for primary education and therefore an increased measurement error correlated with the explanatory take-up variable. However, the relatively small share of the total market this represents suggests this should not present a serious bias problem. Moreover, examination of the pattern for 4-year olds suggests that it is not the case that an increase in children using independent schools for their free early education place as a 3-year old necessarily leads to increased independent school attendance at primary age.

For 4-year olds, the share of free early education places taken in the 'other' sector, and within this, the share taken in independent schools, remain almost constant at approximately 19% and 18% respectively which means that independent schools are accounting for around 3.5% of free early education places taken by 4-year olds, each year[42]. This is important because it reveals that overall the independent schools part of the early education market for 4-year olds is not growing either in terms of its share of the 'other' sector or its share of the free places in total. Therefore this suggests that the measurement error may not be increasing with time, as it is not the case that overall the independent school attendance by 4-year olds is increasing. Secondly, early education for 4-year olds in independent schools is likely to be a stronger predictor of primary education attendance than 3-year olds attendance in independent schools. As the attendance of 4-year olds in early education in independent schools is not increasing, this suggests that the 3-year olds attending independent schools for a free early education place may not necessarily remain there as 4-year olds – the populations in question are approximately the same size as it is approximately the same children just one year later. Thus as the proportions of 4-

[41] It was available in 2002 but I do not use 2002 data due to the missing childcare data.

[42] All of the percentages for 4-year olds are calculated from data in the DfES nursery attendance publications

year olds in independent schools are not increasing it must be the case that some children are attending independent schools for early education when 3-year olds but not when 4-year olds. This indicates that the measurement error problem, which would lead to systematic bias in coefficients if increasing independent school attendance for early education by 3-year olds led to increased attendance in independent schools for primary education, may not be realised.

There is also a caveat concerning the accuracy of the population data obtained from National Statistics. Usually at the level of the LEA, population figures are aggregated to 5-year age bands, and it is unusual for single year of age population estimates to be released – they are done so only with warning as to their accuracy[43]. In the estimation samples that I use, the smallest population figures are 367 for 3-year olds and 366 for 4-year olds, though the next smallest are 1171 and 1156 respectively. Therefore the cell sizes are almost exclusively greater than 1,000 which I consider to be sufficient to be reliable.

It also needs to be considered that the population statistics provided from National Statistics are estimates of resident populations – which does not necessarily mean that the child will attend a provider in the LEA in which they live. The fact that some residents will not use facilities in their home LEA potentially leads to measurement error in the take-up rates – something that is acknowledged by the DfES themselves when they attempt to compute take-up rates (see "Provision for children under five years of age in England January 2000", National Statistics Bulletin for the DfES, Issue No. 01/01, January 2001). However, this is again not something that I can correct in the data, and it is likely to be the case that the majority of pre-school children will attend a place in their own LEA.

2.6.3 Samples

The 1990s saw a number of waves of local government re-organisation in England, the last of which was completed in April 1998, with boundaries remaining constant since then. In parts of the country that still have counties, there is one LEA for each county; elsewhere there is one LEA in each unitary authority, metropolitan district or London borough[44]. As the final changes concluded just after the time of the first year of data that I use, some LEAs are affected. In order to avoid, as far as possible, problems of having to apportion data to new LEAs that did

[43] They cannot be officially termed "National Statistics" as the smaller than usual cell sizes mean that the potential percentage error is larger than is allowed for figures to be declared as "National Statistics". The LEAs that are most likely to be affected by problems of this sort are the smallest LEAs, and these LEAs are much more likely to be excluded from my estimation samples due to other missing data.

[44] The 150 LEAs correspond to 36 Metropolitan Authorities, 33 London boroughs, 47 Unitary Authorities and 34 County Councils.

not exist in 1998 or did exist in 1998 but with different boundaries, I have restricted the main estimation sample to 120 of the 150 LEAs in England. Of the 120 in the main sample, 115 were in existence with entirely the same boundaries and jurisdiction for the entire period from 1998-2003. The remaining 5 LEAs[45] were in existence for the entire time period but had their boundaries changed between the 1998 data and subsequent years. For these latter 5 LEAs, though their boundaries are changed, their early education place data and population data refer to the correct geographical areas so this is not a problem.

The main estimation sample therefore consists of 120 LEAs. For analysis of level 2B or higher results there are 575 observations in total, with each LEA contributing between 3 and 5 observations, with a mean of 4.79 observations per LEA. For level 3 analysis there are a total of 464 observations, with each LEA contributing between 3 and 4 observations with a mean of 3.87 observations per LEA. The reason for the slight unbalancing of the panel is that there are some observations (25 in the 5-year panel, 16 in the 4-year panel) that are data mis-reporting or coding errors resulting in large outlying values, which I exclude.

The exclusion of some LEAs results in the loss of a number of the pathfinder group of 65 LEAs who were first given the NEG for 3-year olds. Remaining in the sample of 120 LEAs are 56 of these most deprived 65 LEAs, thus they make up 47% of the estimation sample of LEAs.

In addition to this main estimation sample, I robustness check my results by also considering a further sample, which includes an additional 10 LEAs who were either boundary changing LEAs[46] or new LEAs in 1998[47] and therefore do not have the consistent boundaries that the main sample LEAs have. I also consider a further sample of "all observations", in which any observations from an LEA that has all the necessary variables for the regression is included regardless of the unbalancing effect on the panel (for details of the samples see Appendix A.1).

The Data Appendix (A.1) contains details of the LEAs included in each sample, and a full list of the pathfinder LEAs that were given the NEG funding first.

2.6.4 Data Descriptives

Tables 2.3 to 2.10 summarise the dependent and independent variables for the main estimation sample, both overall and separately for the two groups defined by whether they received the Nursery Education Grant for 3-year olds in 1999-2000 (i.e. the poorer, 'pathfinder' group) or

[45] Cambridgeshire, Kent, Lancashire, Nottinghamshire and Shropshire.
[46] Cheshire, Devon and Essex.
[47] Blackburn with Darwen, Blackpool, Medway, Nottingham (City), Peterborough (City), Telford and Wrekin, Thurrock.

2000-2001 (the better off, 'non-pathfinder' group).

As the identification in the model comes from within LEA changes in the independent variables affecting changes in the results within an LEA, it is useful to construct the within range of each variable for each LEA. The mean and median within range of each variable is reported in the final column of each table, to illustrate the extent of within LEA variation.

Table 2.3 shows that the mean percentage of children attaining L3 or higher in reading, across all LEAs is 27.00%. The median within range is 5%-points, and we can see from the breakdown of the standard deviation, that the within standard deviation is just under half of the overall, which shows that while there is obviously greater variation in results across LEAs, there is still variation within LEAs over time. For maths at L3 or higher we find a similar level of attainment (26.68% of an LEA's children on average attaining this level) and a similar overall distribution, though with more within variation – the within standard deviation is around two-thirds of the overall standard deviation and the mean and median within range are approximately double (at 10.06 and 10 respectively) the corresponding figures for reading. The mean percentage of children in an LEA attaining L3 or higher in writing is substantially lower, at 13.23%, than is the case for the other two subjects, though there is a quite a lot of variation much of which is within LEA variation over time – the within standard deviation is approximately three-quarters of the overall standard deviation, and at approximately 7%-points the mean and median within range is high compared to the mean attainment at this level.

As would be expected, the percentage of children attaining the expected level – 2B or higher – is much greater for each subject with 68.83 the mean percentage of children in an LEA attaining this level for reading, 59.55% for writing and 73.82% for maths. For each subject however the within variation is lower than for the L3 or higher results, reflected in the within standard deviations and the average within ranges. Appendix A.2 illustrates the results distribution for each subject and level using kernel density plots, with normal curves overlaid – the plots in this appendix's Figure A.1 show that in each case results are close to being normally distributed.

Looking at Table 2.4, we see the same distributions broken down according to whether the LEA was one of the poorer 'pathfinder' LEAs who were first to receive the NEG funding for 3-year olds. While the degree of within variation is generally the same for each group, and so for each mirrors the overall level of within variation for the subject, the means in each group differ as we might expect. For each subject and level, the poorer LEAs have a lower mean attainment than the other LEAs by approximately 6%-points, which is more than one overall standard deviation in each case. The only exception to this is writing L3, which has a much lower mean overall and the difference in means between the two groups is 3%-points which is just under one overall standard deviation. In each case the difference in means between the groups is statistically significant. Figure 2.1 uses kernel density plots to illustrate the distributions of results for each subject and level for the main sample dataset, this time separately for the pathfinder and non-pathfinder LEA groups, with the mean for each group marked in each case. The top row shows that for L3 or higher results, the shape of the distribution for each group of LEAs is similar, with the distribution for the pathfinder LEAs shifted to the left of the distribution for the non-pathfinder LEAs, with the shift being slightly smaller for writing. For L2B or higher results (bottom row), the pictures confirm what the table tells us: that the poorer LEAs' distribution is to the left of the distribution for the better-off LEAs, and also show that the better-off LEAs have a tighter distribution around the mean for each subject at this level, particularly so for maths.

These graphs confirm that there is not anything abnormal about the distributions of results in either group of LEAs, with the poorer LEAs that were targeted for increased early education funding first, having lower results in each subject at each level, looking at the data for the entire time period.

Tables 2.5 and 2.6 respectively show the mean attainment at L2B or higher and L3 or higher for each subject, separately for the two groups of LEAs, by year. The final column shows the difference in means between the groups and these are plotted in Figure 2.2 below. Looking at each group of LEAs separately, it is clear that the means for each subject and level show variation over time, in some cases a large degree of variation – for example maths L3 or higher in 2006 has a dramatic reduction compared with the earlier years. However, interestingly the difference in means between the groups either remains approximately the same or increases. As can be seen clearly in the Figure 2.2 there is certainly no evidence that the gap between the groups of LEAs is narrowing in terms of results in any subject at either level, either as a result of the free early education places policy or any other initiative. If anything, for L2B or higher the gap is increasing in all subjects.

In Appendix A.2, Figures A.2 to A.7 show kernel density plots of the overall distributions (both groups of LEAs together) for each subject and each level, with each year plotted separately. The Figures show that while the shape of results distribution remains approximately the same over time for a given subject and level, there are definite shifts of the distribution left or right dependent on the year. This is particularly the case for the level 3 or higher results. These differences in the positioning of the distributions over time confirm the necessity for including year dummies in the regression model to take account of these movements of the whole distribution over time.

Table 2.7 shows the distribution of the main explanatory variables for the main estimation sample. The mean take-up rate of free early education places for 3-year olds in the maintained nursery and primary schools is 43.09%, though there is a large variation across LEAs – the overall minimum being 1.02% with an overall maximum of 104.26%. These take-up rates in excess of 100% can occur due to the measurement error problems detailed above, mainly because of the possibility of children attending a place in an LEA different to their residential LEA. However, rates in excess of 100% occur in only a small number of LEA-years (5 out of 575). There is much less variation within LEAs overtime than there is overall, though the median within range is 4.37%-points so there is clearly some within variation.

As we would expect, the variation in the take-up rate of free places for 3-year olds in the 'other' sector is very much driven by within variation over-time – since the policy is introduced in two stages over time from an initial base of zero provision, the within variation is necessarily larger and the between variation less. The take-up rate gets as high as 74.53% in one LEA by the end of the time period, but the fact that all LEAs start at zero and do not have any places for the first two or three years weights the mean down to 12.89% overall. The mean and median within ranges are each just over 35%-points indicating the level of take-up that persists on average at the end of the time-period – since each minimum is necessarily zero and in most cases (117 out of 120 LEAs) the highest take-up is in the final year observed.

The take-up rates in nursery and primary school settings for 4-year olds are much higher than is the case for 3-year olds, on average almost double at 81.71%, and while there is variation overall (standard deviation overall is 11.99%-points) this is very much driven by across LEA differences, within variation being much smaller (within standard deviation is 2.12%-points). The mean and median within range of 5.02 and 4.37%-points respectively show that there is within LEA variation. Again there are some take-up rates in excess of 100% due to the aforementioned measurement error issue (19 LEA-years out of 575).

Take-up by 4-year olds in the private sector is much lower than in the public sector, with a mean take-up rate of 15.36%, and some LEA-years where there is no take-up in this sector[48].

[48]This is the case in 22 out of 575 main sample observations. 21 of these were set to zero in the variable's construction due to an implied negative take-up in this sector in the LEA-year (see Data Appendix (A.1) for

Again there is variation (overall standard deviation is 10.31%-points) but it is mainly between LEAs (standard deviation 9.96%-points) rather than within LEAs over time (within standard deviation is 2.69%-points). However, the mean (6.03) and median (4.99) within range show that there is within LEA variation over the time of the panel.

In terms of the provision of childcare places, the table shows that playgroups places (mean 24.41 places per 100 children aged 3 or 4) are much more abundant than the day-nursery (mean 9.20) or childminder places (mean 10.22). Childminder and day nursery place provision have similar overall distributions, though childminder provision variation is more between LEAs with little within-LEA variation, whereas a large part of the variation in day-nursery place provision is within LEAs over time. Playgroup place provision has a much greater range overall and while there is a large amount of between LEA variation, there is within LEA variation with a mean within range of 8.45%-points, median 6.84%-points.

As expected, the overall distributions mask substantial differences in take-up rates and provision between the more and less advantaged LEAs, as can be seen in Table 2.8. It has been documented (Brewer *et al.*, 2005) that more deprived areas have access to fewer private providers and therefore have to rely heavily on the maintained sector, and this is clearly seen in the take-up rates for free early education places for 3-year olds between the more and less deprived LEAs. The mean take-up rate of places in the maintained schools sector in the poorer LEAs is 59.39%, compared to just 28.77% in the better off areas and this difference is statistically significant. The variation is slightly greater overall and between LEAs for the better off LEAs, which is what we would expect given that the better-off LEAs are a more heterogeneous group than the poor LEAs which by definition are all similar in terms of deprivation. As the policy of increasing provision of free early education is differentially targeted towards the more deprived LEAs we would expect that they would have the greater within variation over time and this is what we observe in terms of the average within ranges for this variable for the two groups of LEAs. Figure 2.3 shows for each year, the 25th, 50th and 75th percentiles of the distribution of take-up rates by 3-year olds, of free early education places in the maintained nursery and primary schools, separately for the two groups of LEAs. The Figure illustrates the difference in the distribution of this take-up rate between the groups of LEAs, the poorer, 'pathfinder' group of LEAs, having a much higher take-up and less of a spread, exhibited by the relative closeness of the three percentile lines shown. The take-up rate is lower on average in the better off LEAs

details).

and as we would expect with this more heterogeneous group, there is a greater spread of take-up rates, as can be seen in the right panel of the Figure. Over time the overall distributions for each group of LEAs are very stable with in each case a slight upward trend in the take up rate at each point in the distribution.

For the main variable of interest – take-up by 3-year olds of free early education places in the 'other' sector – the two groups have means that are very similar (13.85% for the poorer LEAs, 12.05% for the better off) and indeed the difference is not statistically significant. However, there are two effects at work: on the one hand the better-off LEAs have access to more private provision, but on the other hand, the poorer LEAs had the NEG funding for 3-year olds a year earlier thus were increasing take-up from zero sooner. Again the mean and median within range provide an indication of average take-up rates at the end of the period since all LEAs begin with zero take-up and take-up is almost always increasing within LEA over time. We can see that the better-off LEAs have a mean within range of 41.79%-points (median 43.71) compared with a mean within range of 28.79%-points (median 30.66%-points) in the poorer LEAs. Thus despite having the funding for free early education places in the private sector a year later than the poorer LEAs, this indicates that the better-off LEAs have over-taken in terms of average take-up in the private sector by the end the of the panel. This pattern is confirmed if we look at Figure 2.4 which shows for each year, the 25th, 50th and 75th percentiles of the distribution of take-up rates by 3-year olds of free early education places in the 'other' sector for each group of LEAs. For 1998 and 1999 (and also for 2000 for the better off LEAs) there were no free early education places in the 'other' sector, hence the horizontal lines at zero for the 25th and 75th percentiles and the median for these years. However we see that the take-up rate shoots up to a median of 16.6% in 2000 for the poorer LEAs and continues to increase year on year thereafter. The better off LEAs receive the funding for the first time in 2001 and their median take-up in this sector rises to 9.7% in this year – which is below the corresponding figure of 25.2% for the poorer LEAs in 2001. However, by 2002 the better off LEAs have a higher take-up rate at each point of the distribution shown here, and the gap is massively increased by 2003, with the median take-up rate in this sector for the better off LEAs reaching 43.7%. This graphic dramatically illustrates the way in which the better off LEAs quickly surged ahead in terms of the take-up of what are largely privately provided early education places.

As is the case with 3-year olds, the more deprived LEAs have a greater take-up of maintained sector places by 4-year olds (88.08% mean take-up in the more deprived LEAs, 76.10% in the less), and though this difference is statistically significant, it is not as stark a contrast. Though the better-off LEAs have greater access to private provision of early education, by the age of 4 many children are attending the reception or infant class of the primary school that they will attend, and as private school attendance is much lower than maintained school attendance – particularly at primary level – the difference in take-up by 4-year olds of pre-compulsory school places between the poorer and better-off LEAs is not nearly as large as is the case when the children are 3. Again the between LEA variation is greater for the better-off LEAs while the poorer LEAs have slightly greater within variation both in terms of the average within range and also the within standard deviation.

As the Nursery Education Grant funding for private places for 4-year olds was available to all LEAs from the start of the panel, it is not surprising to see that the better-off LEAs have a greater take-up rate of private sector places, more than double the average take-up rate of the poorer LEAs (20.36% versus 9.69%), a difference that is statistically significant. There is more between LEA variation amongst the better-off LEAs and also there is slightly more within LEA variation amongst these LEAs too, though again we may expect this to be the case as take-up of free early education by 4-year olds in the private sector is likely to be associated with private school attendance for primary school which we expect to see less of, and see less change in, for the poorer LEAs.

In terms of childcare places provided, while the day nursery and childminder place provision is similar in the two groups of LEAs, slightly less provided in the poorer LEAs as we would expect (though not a statistically significant difference for day nursery places), it is playgroup place provision where the difference is large. The better off LEAs provide on average 31.22 playgroup places per 100 children aged 3 or 4, compared with an average of just 16.65 in the poorer LEAs. This may explain why the take-up of free early education places by 3-year olds in the maintained schools sector is much lower in the better off LEAs, if many are using childcare settings rather than more formal education settings at age 3.

Table 2.9 contains the descriptive statistics for the additional control variables included in the regressions when I allow differing effects for the poorer and better-off LEAs. This is then broken down for the two groups of LEAs in Table 2.10. As we would expect, the poorer LEAs have a higher economic inactivity rate amongst working age males (19.66% versus 13.05%) and the difference is statistically significant, while the variation between LEAs and within LEAs over time is similar for each group. The average manufacturing occupations rate and the average of median male worker weekly gross pay however are not statistically different between the two groups of LEAs and indeed the poorer LEAs actually have a higher average of median male worker weekly gross pay. We can see that within LEA variation is very similar for this variable in the two groups but between LEA and overall variation is greater in the poorer LEAs – this is because of a small number of LEAs in the poorer group who nevertheless have high levels of median gross income[49]. The earnings measures available in my data are not ideal in that they are based on workplace rather than residential location. This allied to the fact that some of areas that qualified for the early NEG funding as a 'deprived' LEA actually have some very prosperous areas in addition to very poor areas, means that we get this slight anomaly. This means that a few large outliers skew the data – while the mean of median male gross weekly pay is higher in the poorer LEAs, the median of this variable is lower: £389.80 in the poorer LEAs versus £401.95 in the better-off LEAs.

In terms of average KS1 class size, the groups are almost identical in mean (25.65, poorer LEAs; 25.63 better-off) and also in terms of the variance of the distribution both between and within LEAs.

It is noticeable that the poorer LEAs have a significantly higher average percentage of non-white children in their maintained schools: 27.38% compared with 10.75% in the better-off LEAs. There is slightly more variation in the poorer group of LEAs both across and within LEAs, with some poorer LEAs having a particularly high concentration of non-white children – the highest overall in any LEA-year being 80.91%, and the highest time-average for a poorer LEA being 77.47%, compared with the corresponding figure of 58.64% for the better-off LEAs. It is important therefore to include these covariates to control for any potential effects that this may have on KS1 results.

2.7 Results

2.7.1 Simple Policy Dummy Analysis

The initial model is a panel regression with a simple [0,1] policy dummy for the years in which the policy is in operation; the results are displayed for reading, writing and maths at each level, in model #1 columns of Tables 2.11, 2.12 and 2.13 respectively. Looking at the tables we see that, despite the introduction of the policy at two different time points, we cannot identify a significant effect of the policy introduction on reading, writing or maths results at either level.

[49] Tower Hamlets, Southwark, Camden, Westminster, Islington, Hammersmith and Fulham.

At level 2B or higher the estimated policy effect is almost zero for reading, while for writing the coefficient is +0.255 indicating that the policy had the effect of increasing the percentage of children attaining level 2B or higher by 0.255%-points, though it is not significantly different from zero. Similarly for maths, the estimated coefficient of -0.250 suggests that the policy reduces attainment at level 2B or higher by a quarter of one percentage point though again it was not significant. The year dummies are highly significant in all years for writing and in two of the four later years for reading and for maths, and when this common year variation is removed, it appears that there is not sufficient variation in results within LEAs to identify the effect of the policy.

The story is similar for level 3 or higher results, each of the year dummies are significant at the 1% level for each subject, and once this common variation is removed, the remaining variation in results is not sufficient to identify the policy effect.

Appendix A.3 shows the results of the model for the robustness check samples and confirms that we do not get any significant coefficients for either level in any of the samples, though there is generally a consistency to the point estimates for each subject and level across the samples.

The second specification of the policy evaluation model allows the policy effect to differ according to whether the LEA was one of the poorer LEAs that had the policy implemented in the first phase of its introduction. The results for this model can be seen in Tables 2.11, 2.12 and 2.13 but under the model #2 columns. We can see that for level 2B or higher reading and writing, we continue to fail to identify policy effects in either group of LEAs. It is interesting that while not significant estimates, for writing at this level there is a positive effect of the policy for each group of LEAs, whereas for reading the effect is estimated to be negative for the poorer LEAs but positive for the better off LEAs. This pattern for reading is also witnessed in the maths results at level 2B or higher, but for maths while the positive effect for the better off LEAs is not statistically significant, the negative effect for the poorer LEAs is significant at the 5% level. The estimate of -0.772 suggests that in the poorer areas, the policy led to a 0.772%-point fall in the percentage of children attaining level 2B or higher in KS1 maths. To put this in context, the average attainment at this level in the poorer LEAs is 70.79% with a within standard deviation of 2.02%-points. Therefore the policy effect is equal to a reduction of approximately 1/3 of a within standard deviation. Table A.6 in Appendix A.3 shows that this result is consistent across the samples, remaining of similar magnitude and significant at the 5% level and in one sample at the 1% level.

Turning to the level 3 or higher results for model #2, Table 2.11 shows that for reading there is a positive effect of the policy for each group and in the case of the poorer LEAs there is a marginally significant (p=0.112) coefficient of 0.425 estimated. This suggests a 0.425%-point increase in the policy years for these poorer LEAs. The mean attainment in reading at this level in these LEAs over the period is 23.41 with a within standard deviation of 1.90, therefore the policy effect is equivalent to approximately one fifth of a within standard deviation of results for these LEAs.

Similarly for writing level 3 or higher we see from Table 2.12, that there is a marginally significant (p=0.107) coefficient of -0.728 estimated for the policy effect in the poorer LEAs. This suggests that the policy decreased the average percentage of children in the poorer LEAs' maintained schools attaining level 3 or higher in writing by 0.728%-points, which is approximately one quarter of a within standard deviation (2.76%-points), with the mean being 11.39%. The effect for the better off LEAs is positive but not significant.

Table 2.13 shows that for maths at level 3 or higher, while the policy effect for the better off LEAs is not significantly different from zero, the policy effect coefficient estimated for the poorer LEAs is -0.825 and is significant at the 5% level. This suggests that the policy led to a 0.825%-point reduction in the percentage of children in the poorer LEAs' maintained schools attaining level 3 or higher in KS1 maths, which is approximately one fifth of a within standard deviation (4.03%-points), while the mean is 23.50%.

Appendix Tables A.7, A.8 and A.9 show that these results are robust to the choice of sample and in many cases strengthen in significance in the alternative samples. Moreover, the Appendix A.3 Tables A.10 to A.15 show that all of these results remain when we exclude the 2006 data on account of the change in assessment method introduced in 2006.

It appears that there is a significant policy effect on results but that it is only in evidence when we allow the policy to have different effects in the two groups of LEAs, and the effect is only in the poorer LEAs. While there is a slightly significant positive effect on reading level 3 or higher in the poorer LEAs, for writing and maths it is a significant negative effect of comparable or greater relative magnitude. The negative policy effect on maths results in the poorer LEAs is also in evidence in attainment at level 2B or higher. These are interesting results and slightly alarming in that the policy was designed to be a benefit to the poorer LEAs in particular and it is in these areas that the policy is having a significant and mainly negative impact on results. To investigate further I estimate the more complex model, exploiting variation in the actual take-up rates to examine the way in which this policy is working.

2.7.2 Policy Effects Through Early Education Place Take-up

Looking at the results from first specification of the more complex model in Tables 2.14 – 2.16, we see that despite the fact that the expansion of the 'other' sector provision of free early education places began at different times in the two groups of LEAs, there is a failure to identify any effect of this expansion on results in reading, writing or maths at either level 2B or higher or level 3 or higher. Indeed, aside from the year dummies, there are very few coefficients that are significant in any regression. It appears that, as with the analogous basic policy evaluation regressions, the variation common to all LEAs according to the year of the test, dominates other differences in results to such an extent that we cannot identify any significant parameters.

The exception to this is the effect of the take-up, by 3-year olds, of free early education places in maintained nursery schools and nursery classes in maintained primary schools on the proportion of children achieving level 2B or higher in reading. As can be seen in Table 2.14, we find a significant positive effect, though the coefficient of 0.037 is very small – a 10%-point rise in the 3-year olds take-up rate of these places would lead to a 0.4%-point increase in the percentage of children achieving level 2B or higher at KS1 reading. To put this in context, over the time of the panel, the mean take-up rate by 3-year olds of free early education places in maintained nursery schools and nursery classes in maintained primary schools is 43.09% and the mean percentage of children attaining level 2B or higher in reading is 68.83% with a within standard deviation of 1.70. Therefore the effect is equivalent to an increase in attainment of approximately one quarter of a within standard deviation.

Appendix A.4 Table A.16 shows that this result is robust to the choice of sample. The result is also robust to the exclusion of the childcare variables (see Appendix A.5 Table A.23), which allows the panel to be extended by one year – 2002 data can be used as it is only the childcare variables that are missing in this year (see Appendix A.5 for details).

As discussed in section 2.5, the introduction of the policy to make mandatory the provision of free early education places to all 3-year olds, was a shock to the whole market and therefore the influence may be felt not only through the places in the 'other' sector that were taken up from an initial position of zero take-up, but also through the maintained schools sector take-up. However, while it is true that the selection into the maintained schools' places as opposed to the 'other' sector is no longer on the basis of cost, since all places are free, in order to be able to interpret the effects of increased take-up in each sector as causal I make the assumption that there is not systematic selection on other unobservable parental tastes. The estimated fixed effects deal with any time-invariant heterogeneity *between* LEAs, however *within* an LEA, it is necessary to assume that allocation to places in each sector is not correlated with unobserved parental characteristics.

While it is disappointing not to find more significant effects at this stage, an important part of this study involves evaluating whether there are differential effects of the changing balance between public and private providers of free early education places, according to the level of deprivation of the LEA. Tables 2.17 – 2.19 display the results of the regressions when I allow the effect of each of the early education take-up rate variables to vary according to whether or not the LEA was one of the poorer LEAs who were allocated the NEG first. Many of the control variables to capture selection into the first group of LEAs to receive the NEG did not prove to have any significant effect, thus in the interests of parsimony have been dropped.

2.7.3 Reading

In the policy evaluation regressions when we allow the policy effect to differ for the 'poorer' and 'better off' LEAs we found the effect on reading level 2B or higher was negative for the poorer LEAs, positive for the better off LEAs, though in neither case significant. Table 2.17 shows that the coefficients on take-up in the non-school sector, that is the 'other' sector, follow this same pattern as we would expect, however again neither effect is significant.

What we do find is that it is the poorer LEAs that are driving the result that increasing the take-up by 3-year olds of free early education places in maintained nursery and primary schools has a positive effect on achievement in reading at level 2B or higher, as Table 2.17 shows. The coefficient is still small at 0.059, implying that a 10%-point rise in the take-up rate of these places by 3-year olds in the poorer LEAs would lead to a 0.6%-point increase in the percentage of children achieving level 2B or higher at KS1 reading. The mean take-up of these places by 3-year olds in the poorer LEAs is 59.39% and the mean percentage of children achieving level 2B or higher in reading in these LEAs is 65.68% with a within standard deviation of 1.66. Therefore the effect of a 10%-point increase in take-up would be equivalent to an increase of approximately one-third of a within standard deviation for these LEAs. Appendix A.4 Table A.17 shows that this result is robust to the choice of sample, and the result is also robust to the exclusion of the childcare variables (see Appendix A.5, Table A.24).

As with the policy evaluation regressions, a further robustness check involves running the regression again but with the data from results year 2006 excluded. The assessment method for each of the subjects changed in 2006 hence the need to consider excluding 2006 as the results may be differentially biased in 2006, meaning that a dummy for 2006 would not control for this effect. Appendix A.6 Table A.30 reveals this result remains significant and robust to the choice of sample; indeed the coefficient increases slightly and is significant at the 1% level as opposed to just the 5% level, which is the significance level when we include 2006 data.

Turning to the effects at level 3 or higher, the marginally significant positive policy effect for the poorer LEAs that we found in Table 2.11 is not in evidence in Table 2.17 – we find the coefficient on 'other' sector take-up to be estimated to be zero to 3 decimal places.

However, we do see in Table 2.17 that there is some evidence that take-up of free early education places in maintained nursery and primary schools by 4-year olds in the better off LEAs, has a positive effect on the percentage of children in an LEA attaining level 3 or higher in their KS1 reading. The coefficient of 0.159 indicates that an increase in the 4-year olds take-up rate of these places in the better off LEAs is associated with a 1.59%-point increase in the percentage of children attaining level 3 or higher. To put this in context, the mean take-up rate of free places in the maintained nursery and primary schools by 4-year olds in these less deprived LEAs is 76.10%, and the mean percentage of children attaining level 3 or higher in these LEAs is 30.15% with a within standard deviation of 2.14. Therefore the effect of a 10%-point increase in take-up is approximately equal to an increase of just under three quarters of a within standard deviation.

Though this result is found in both of the balanced panel samples (see Appendix A.4 Table A.20), the level of significance increases to 10% in the second sample, and the result is not robust to unbalancing the panel (sample 3). When the childcare variables are excluded the result remains but only in the main sample (see Appendix A.5, Table A.27). As can be seen in Appendix A.6 Table A.33, when we exclude the 2006 data, this result becomes much stronger, becoming significant at the 1% level in both the main and second balanced panel samples.

It is notable that some of the variables that are included to control for selection into the group of LEAs who received the NEG for 3-year olds first, are significant in each regression. In the level 2B or higher regression the male economic inactivity rate amongst working age males is significant and negative in each sample, which is what we would expect to be the case – as the inactivity rate increases the results for children in these LEAs decrease. Similarly, there is a negative coefficient on the proportion of men and women who are employed in manufacturing occupations for results at level 2B or higher. This again is the sign that we would perhaps expect – to the extent that manufacturing jobs capture socio-economic status of the LEA, as the proportion of manufacturing jobs increases we might expect that results would decrease.

In the regressions for level 3 or higher, the male economic inactivity rate is no longer significant, and neither is the manufacturing jobs rate (which in the interest of parsimony is dropped from the specification). The median gross weekly pay of male workers is significant and had a positive effect, again as we would expect – as the pay of workers increases, the results of children in the LEA increase.

It is in line with what we would expect that factors contemporary to the time that the children take the test have a larger impact on results than factors from earlier in the lifetime of the children – and it is clear that average class size for KS1 classes seems to have a significant effect on results for reading at each level. As we would expect, as the average class size increases, results decrease: increasing the average class size by 1 child has the effect of decreasing the percentage of children attaining level 2B or higher by 0.67%-points and the percentage of children attaining level 3 or higher by 0.78%-points. As the mean percentage attaining level 2B across all LEAs is 68.83% with a within standard deviation of 1.70 and the corresponding figures for level 3 are 27.00% and 2.03, it is clear that even changing average class size has only a relatively small effect on results.

2.7.4 Writing

In the policy evaluation regressions when we allow the policy effect to differ for the two groups of LEAs, for both there was a positive policy effect on results at level 2B or higher but in each case it was not significant. The effect of the 'other' sector place take-up continues to be non-significant for each group of LEAs in Table 2.18, with the coefficient for the poorer LEAs actually being slightly negative though very close to zero.

While there were no significant results for writing in the regressions estimating common effects for the two groups of LEAs at level 2B or higher, Table 2.18 shows that when we allow for different effects we find that there is evidence of a small positive effect of take-up of free early education places in maintained nursery schools and nursery classes in primary schools by 3-year olds in the poorer LEAs, on the percentage of children attaining level 2B or higher in writing at KS1. As with the result for reading, the coefficient is very small at 0.07, indicating that a 10%-point increase in take-up by 3-year olds in these poorer LEAs would result in a 0.7%-point increase in the percentage of children attaining level 2B or higher. As outlined above, the mean take-up rate by 3-year olds of free early education places in maintained nursery and primary school settings in these LEAs is 59.39%, while the mean percentage of children in these LEAs attaining level 2B or higher in writing is 56.30% with a within standard deviation of 2.19. Therefore a 10%-point increase in take-up is associated with an increase in attainment at this level equivalent to approximately just under one-third of a within standard deviation. This result is robust to the choice of sample (see Appendix A.4, Table A.18), whether or not we include 2006 data (see Appendix A.6, Table A.31) and whether or not we include the childcare variables (see Appendix A.5, Table A.25). There is some sensitivity to sample choice when we exclude the childcare variables and the 2006 data. The level of significance is at least 5% for all samples when 2006 data is included, drifting to 10% when 2006 data is excluded.

The first main result therefore, is that there is a small but significant and robust effect of take-up of free early education places in maintained nursery schools and nursery classes in maintained primary schools, by 3-year olds in the poorer LEAs, on the percentage of children achieving level 2B or higher in KS1 reading and writing. It is perhaps not surprising that settings that increase results for reading also affect results for writing given the obvious complementarity between the two skills, and it appears that in the poorer LEAs there is a positive effect of increasing take-up of free early education in the state maintained schools sector.

Turning to level 3 or higher results, in the policy evaluation regressions when we allow the policy effect to differ for the two groups of LEAs, we found a significant negative policy effect on results at level 3 or higher for the poorer LEAs, and a non significant positive effect in the better off LEAs. Table 2.18 shows that the signs on the estimated coefficients for take-up of 'other' sector places by 3-year olds in each group of LEAs concur with the policy evaluation regressions though in neither case are they significant.

We also see in Table 2.18 that there is some evidence that take-up of free early education places in maintained nursery and primary schools by 4-year olds in the better off LEAs, has a positive effect on the percentage of children in an LEA attaining level 3 or higher in their KS1 writing. The coefficient of 0.157 indicates that a 10%-point increase in the take-up rate of these places by 4-year olds in the less deprived LEAs is associated with a 1.57%-point increase in the percentage of children attaining level 3 or higher. To put this in context, as outlined above, the mean take-up rate of free places in the maintained nursery and primary schools by 4-year olds in these less deprived LEAs is 76.10%, while the mean percentage of children attaining writing level 3 or higher in these LEAs is 14.84% with a within standard deviation of 3.19. Therefore the effect of a 10%-point increase in take-up is approximately equal to an increase of half of one within standard deviation. As with reading at this level, though this result is found in both of the balanced panel samples (see Appendix A.4, Table A.21), the level of significance increases to 10% in the second sample, but unlike in the case of reading, the result is significant at the 5% level in the unbalanced panel sample. As with the corresponding effect on reading, when the childcare variables are excluded the result remains, but only in the main sample (see Appendix A.5, Table A.28). Again mirroring the case of reading at this level, when we exclude the 2006 data, the result becomes stronger, becoming significant at the 5% level in the sample 2 and remaining so in the main sample and the unbalanced panel sample (see Appendix A.6, Table A.34).

Therefore, the second main result is that there is a significant and robust effect of take-up of free early education places in maintained nursery and primary schools, by 4-year olds in the better off LEAs, on the percentage of children achieving level 3 or higher in KS1 reading and writing. Again we see a common positive effect – almost identical in terms of the coefficient – on reading and writing, of the state maintained nursery and primary school settings providing free early education places, though in this case it is the higher level of attainment and the better off LEAs.

It is also clear that for reading and writing regressions, the extra variation that comes from including the actual take-up rates of free early education places in the ‘other’ sector rather than just a simple dummy for the policy implementation does not lead to greater identification of the policy effects. As shown in Tables 2.17 and 2.18, we cannot identify the effect of the introduction of these non-school sector free places for results in either subject or at either level.

In the writing regressions, the covariates included to attempt to control for selection into the group of LEAs that had the policy implemented earlier, are not always significant or of the expected sign. In the regression for level 2B or higher attainment both the male economic inactivity rate and the manufacturing jobs rate have negative coefficients of similar magnitude to the corresponding coefficients in the reading regression at this level, however in the case of writing, they are not quite significant. In the regression for level 3 or higher, the male wage and economic inactivity variables – that were included for the reading regression at this level – are not anywhere near significant and therefore dropped from the model in the interest of parsimony.

However, the manufacturing jobs rate is included and has a significant positive coefficient. The sign is in contrast to the case for reading at this level and is not what we would expect to be the case.

In the regressions at each level, we find that the average class size for KS1 enters again with a significant negative coefficient. For level 2B or higher the coefficient is -0.948 suggesting that for every extra child added to the average class size, the percentage of children attaining this level decreases by approximately 1%-point. While the coefficient on average KS1 class size from the level 3 or higher regressions (-0.450) suggests that for every additional child added to the average class size, the percentage attaining level 3 or higher falls by less than half of one percentage point. Since the mean percentage of children attaining level 2B or higher in writing, across all LEAs is 59.55% with a within standard deviation of 2.10, the effect is approximately half of one within standard deviation. For level 3 or higher it is relatively even smaller since the mean percentage attaining this level across all LEAs is 12.23% with a within standard deviation of 2.99, thus the effect of increasing average class size by 1 child is less than one sixth of a within standard deviation. Therefore, similarly to the case of reading, average KS1 class size has a significant and robust effect on all levels of writing attainment, though again the effects are relatively small in magnitude.

2.7.5 Maths

In the policy evaluation regressions with differing effects for the poorer versus the better off LEAs, there was a significant negative policy effect for the poorer LEAs on attainment at level 2B or higher, and a positive but not significant effect for the better off LEAs. As we can see from Table 2.19, the coefficient on take-up in the ‘other’ sector for the poorer LEAs is negative but is not quite significant. However, the positive coefficient on take-up of ‘other’ sector places by 3-year olds in the better off LEAs is significant at the 5% level. In magnitude the coefficient is very small at 0.018, indicating that a 10%-point increase in the percentage of 3-year olds in these LEAs taking a free early education place in a setting in the ‘other’ sector would lead to an increase of less than one-fifth of one percentage point in the percentage of children attaining level 2B or higher in maths. The percentage of children attaining this level in maths in the better off LEAs is 75.48% with a within standard deviation of 1.66. The effect of a 10%-point increase in take-up is therefore equivalent to just under one ninth of a within standard deviation. Moreover, while the result may be robust to the exclusion of the childcare variables (see Appendix A.5,

Table A.26), it is not robust to the choice of sample (as can be seen in Appendix A.4, Table A.19). Furthermore, the result is not robust to the exclusion of 2006 data (see Appendix A.6, Table A.32).

At level 3 or higher, the policy evaluation regressions found a significant negative policy effect in the poorer LEAs. Table 2.19 shows that we do still pick up this effect in the coefficient on 3-year olds take-up of 'other' sector places in the poorer LEAs, though the coefficient of -0.025 is just outside the 10% level of significance (p=0.102). Though this result is not robust to the choice of sample (see Appendix A.4, Table A.22) or the exclusion of the childcare variables (see Appendix A.5, Table A.29), it is robust to the exclusion of results year 2006 data (see Appendix A.6, Table A.35) in which case it is significant at the 10% level. The coefficient suggests that a 10%-point increase in 'other' sector take-up in the poorer LEAs would decrease the percentage of children attaining level 3 or higher by a quarter of one percentage point. To put this in context, the mean take-up rate of 'other' sector places amongst 3-year olds in these LEAs is 13.85% and the mean percentage of children attaining level 3 or higher in maths in these LEAs is 23.50% with a within standard deviation of 4.03, so the effect is around one sixteenth of a within standard deviation.

Table 2.19 also shows that there is some evidence of an effect of take-up of free early education places in maintained nursery and primary schools by 4-year olds in the better off LEAs on results in maths at level 3 or higher. The coefficient of 0.155 suggests that a 10%-point increase in the take-up of places in these settings by 4-year olds in these LEAs is associated with a 1.55%-point increase in the percentage of children attaining level 3 or higher in maths. To provide some context, the mean take-up rate in these school settings amongst 4-year olds in these LEAs is 76.10% and the mean percentage of children attaining level 3 or higher in maths in these LEAs is 29.48% with a within standard deviation of 4.15, thus the effect is equivalent to just over one third of a within standard deviation. It is noticeable that the coefficient is very similar to the effect that we find for both reading and writing attainment at level 3 or higher in these better off LEAs.

The result is largely robust to the choice of sample (see Appendix A.4, Table A.22) and also to the exclusion of the childcare variables (see Appendix A.5, Table A.29) – indeed the result is much stronger in all samples in this case. When excluding the results year 2006 data the coefficient is just outside the 10% level of significance (p=0.103) in the main estimation sample (see Appendix A.6, Table A.35).

Therefore there is some evidence to suggest that the positive effect that we see for reading and writing of early education places being taken in the maintained nursery and primary school settings by 4-year olds in the better off LEAs is also present in maths – and of a similar magnitude.

At each level, the covariates included to control for selection into the group of LEAs who received the NEG first, are generally not significant. At level 2B or higher there is a slightly significant effect of the manufacturing jobs rate, with a negative coefficient as we may expect. However this is the only such variable that is significant and this is only at the 10% level and only in the main sample. Including different variables in different combinations as controls does not yield any significant coefficients for these variables. Similarly, at level 3 or higher, the particular covariates we use as controls does not seem to make a difference – in the Tables 2.19, A.22 and A.35 we use the same covariates as we use for the reading level 3 or higher regressions and none are significant in any sample. Including different combinations of control variables does not yield any significant effects of these variables. As we know from the reading and writing regressions that there is variation in these control variables – it is not simply the case that they do not vary sufficiently in any LEAs to be able to obtain any identification – their non-significance in the maths regressions leads me to conclude that they are not having any significant impact on the results in maths and therefore are not biasing the coefficients for either group.

As with the reading and writing regressions, the average class size at KS1 has a significant negative effect on results at both levels for maths. For level 2B or higher, an increase of 1 child in the average class size has the effect of reducing the percentage of children attaining this level by 0.425%-points, which is just under one quarter of a within standard deviation (1.84). For level 3 the effect is to reduce the percentage achieving this level by 0.726%-points, which is just over one-sixth of a within standard deviation (4.09).

We can see that the proportion of children in the LEA at the time of the KS1 tests, who are from non-white ethnic origin has a significant (at 1% level) negative effect on the proportion of children attaining level 2B or higher in maths, though this only seems to affect level 2B or higher as there is no measured effect on level 3 or higher attainment.

2.7.6 Discussion of the Implications of these Results

The policy evaluation regressions show that there is a significant effect on some subjects' results of the policy introduction but that it is not universal – the effect is different in the two groups of LEAs and only significant for the poorer LEAs. Common variation due to the year effects accounts for much of the variation in results, with the remaining local variation insufficient to identify a common policy effect in the simplest regressions. In several cases the policy has a different signed effect between the groups and it appears that these effects counter act each other such that when estimated as a single effect it is not significantly different to zero. In other cases it is only a marginally significant effect in the poorer LEAs and not significant in the better off LEAs such that when estimated as a single parameter the poorer LEAs' effect is not strong enough to identify a significant overall effect.

In terms of evidence of the policy effects working through the actual take-up rates of 'other' sector places themselves, it is only in maths scores that we see the significant policy effects mirrored by the significant 'other' sector take-up rate impacts. In the better off LEAs there is a positive (though not significant) policy effect for maths at level 2B or higher and this is echoed and significant in the main sample estimation of the effect of the take-up rate of 'other' sector places in these LEAs. For level 3 or higher results the policy evaluation regressions suggest a negative policy effect on results in the poorer LEAs, and this effect is identified in the coefficient on the take-up of 'other' sector places in these LEAs, in the more complex model regressions. However, these maths results are sensitive to the choice of sample and do not remain in all of the other robustness checks.

Looking at the more indirect effects, it appears that for both reading and writing the maintained nursery and primary school settings have a small but significant positive effect on attainment at the intermediate level for the poorer LEAs when accessed by 3-year olds. These settings are also associated with a larger and significant positive effect on attainment at the higher level for the better off LEAs when accessed by 4-year olds.

However, for 4-year olds it is not possible to interpret this as a causal effect. The introduction of free early education places for 3-year olds was a shock to the market for childcare and early education for 3-year olds but much less so for 4-year olds. The choice of setting for 4-year olds is likely to be much more influenced by the consideration of where the child will be for their primary school education – the nursery and primary school settings for 4-year olds include the reception class, which children can attend for between one and three terms depending on the choice of the parents and the birthday of the child. Table 2.20 shows the percentage of 4-year olds attending a nursery or primary school that are in an infant or reception class as opposed to a nursery class, for each region of England for 1998-2002[50]. It is clear that a large majority of 4-year olds in nursery and primary school settings are in fact in an infant or reception class in the primary school – and thus in the school that they will attend for their primary education. Therefore the choice to send the child to a maintained school setting rather than a private setting at age 4 is likely to be strongly influenced by the parents' preferences over primary schools and therefore it is not appropriate to assume that within the LEA, allocation of 4-year olds to school or 'other' sector settings is uncorrelated with unobservables. We would suspect there could be a significant upward bias in the estimated effect: parents with strong preferences over their child's education are likely to try to get their 4-year old into a reception class in a particular, good, primary school rather than use a free place in a private nursery setting. The children of these parents are also more likely to achieve higher results due to the parental preferences over education, leading to an upward bias in the coefficient on 4-year olds attendance of maintained school places.

It is worth noting that this argument does not apply to the case of 3-year olds since it is policy that attendance at a maintained school as a 3-year old does not have any influence on the probability of gaining a place in the reception intake at that school. Thus the link between early education place setting and primary school place does not exist for 3-year olds in the way that it does for 4-year olds and is therefore much less likely to create a selection problem. In theory the early education places for 3-year olds in the school and in the 'other', non-school, settings are identical so a parent with strong educational preferences should view them as equal and not select on this into either particular sector.

The effect of maintained school settings' attendance by 3-year olds in the poorer LEAs on their reading and writing results is in line with what our prior may be. I am comparing the effect of attendance in one of the settings with staying at home and not attending any setting and it appears that, compared with staying at home, a greater proportion of the LEA's children attending a maintained nursery setting aged 3 is associated with more children attaining the expected level (i.e. at least level 2B) on their KS1 assessment. If we expect that the average home environment for children in these poorer LEAs is less educationally beneficial to a child than attendance part-time in a nursery school setting, then we would expect that increasing take-up by 3-year olds in these LEAs would be associated with positive effects on child outcomes. Given that reading and writing in particular are skills more readily advanced through nursery school

[50] 2001 is missing due to data non-collection in this year, this is also the case for 2004.

attendance (as opposed to maths skills), we might expect that we would find effects on results in these areas.

If we believe that for the better off LEAs, the quality of home inputs is on average better, then it may be no surprise that attendance in more formal settings aged 3 or 4 does not seem to affect the intermediate level of attainment but attendance at age 4 does have a significant effect on attainment at the higher level – and for maths also. As discussed, there is likely to be an upward bias in the coefficient due to selection. However, it is also consistent with the idea that children who develop skills early can be encouraged to develop further and beyond expectations in the more formal nursery and primary school settings whereas this may be more difficult at home.

It is noticeable that the effects at level 3 are greater in magnitude and relative size than the results at level 2B and again this could be due to an upward bias but is also in line with what our prior may be. Bearing in mind that I am unable to distinguish between no effect at all and an effect that is mitigated by the first two years of formal schooling, it is perhaps to be expected that the first years of schooling would do a better job of mitigating any advantage that nursery attendees had over their home-staying class mates at the intermediate level, to the extent that the measured effect on KS1 results whilst statistically significant in the poorer LEAs, is relatively small. For the higher level results, it may be more likely to be the case that the advantage of early exposure to the formal educational setting remains, if this gives the platform on which to build – with the initial schooling having a greater gap to make up if these children in better off LEAs who attend early education places are in a better position to build on their early advantage. As Table 2.20 shows that a large proportion of 4-year olds in nursery and primary school settings are in fact in an infant or reception class in the primary school, the "result" for 4-year olds suggests that results are better at the higher levels for reading, writing and maths in the better off LEAs when more children start school at age 4 rather than remaining at home aged 4.

It has to be borne in mind that the measurement error issues outlined in section 2.6, would suggest that the estimated coefficients are lower bounds for the estimates of the true parameters. However, even taking this into account, in terms of policy implications conclusions are slightly tempered by the magnitudes of the robust results that I find. As discussed above, if the take-up of early education places in maintained nursery and primary schools, by 3-year olds in the poorer LEAs, could be raised by 10%-points, the effect would be to increase the percentage attaining level 2B or higher in reading by just 0.6%-points and in writing by 0.7%-points. Over the time of the panel, the mean percentage of children in these poorer LEAs attaining level 2B or higher in reading is 65.68% with a within standard deviation of 1.66%-points - so we can see that even a 10%-point change in take-up is making only a relatively small impact. Similarly the mean attainment in these LEAs for writing level 2B is 56.30% with a within standard deviation of 2.19%-points thus even more so for writing, the impact of increasing take-up even by 10%-points is relatively small.

One way in which to assess these findings is to consider what the estimated effect would be, *ceteris paribus*, if all of the 3-year olds in the poorer LEAs who attend 'other' sector places switched to maintained schools places. At the end of the panel, the median take-up of places in the 'other' sector by 3-year olds in the poorer LEAs was 30.29% (mean 28.77%). If the take-up in the maintained schools sector increased by 30%-points the effect on reading level 2B would be to raise results by approximately 1.8%-points which is just over one within standard deviation. For writing level 2B the effect of a 30%-point increase in maintained school place take-up would be an increase of approximately 2.1%-points which is just under one within standard deviation. Therefore this admittedly basic calculation (not taking into account capacity constraints or allowing non-linear effects in increasing take-up) suggests that expanding the maintained schools provision of places for 3-year olds in the poorer areas, in the stead of 'other' sector places, would lead to a significant increase in reading and writing results.

One other way in which to assess the size of these significant parameters, is to compare the effect of a 10%-point increase in the take-up rate of free early education places in the maintained schools sector, by 3-year olds in the poorer LEAs, with the effect of reducing the average KS1 class size by one child[51].

The effect of increasing take-up by 3-year olds in the poorer LEAs by 10%-points, would for reading level 2B or higher, have an effect equivalent to reducing the average KS1 class size by one child. Put in this context, we can see that though the effect of raising this take-up rate seems very small, it is of the same order as reducing class size by one child. For writing level 2B or higher, the effect of increasing take-up rate by 3-year olds in the poorer LEAs by 10%-points is not quite as great in comparison to reducing the average class size. Such an increase in take-up rate would correspond to a reduction of approximately three-quarters of one child, or put another way, it would take an increase in the take-up rate by 3-year olds in the poorer LEAs, of

[51] Clearly this exercise makes the simplifying assumption that there is a monotonic effect of reducing KS1 class sizes, which in reality is not likely to be the case.

approximately 14%-points to have an effect on results equivalent to a reduction by one child in the average KS1 class size. Again, in this context, the seemingly small effects of an increase in 3-year olds in poor LEAs taking their free early education place in maintained schools settings, is seen to be of more substantial size than initially thought.

Moreover, these results are in line with the EPPE study which found that nursery schools and classes were the better pre-school environments for 3- and 4-year olds to attend prior to compulsory schooling, which adds to their credence.

2.8 Conclusions

The aim of this study was to evaluate whether the policy of introducing free early education places for all 3-year olds has had any impact on results at Key Stage 1, and moreover to see whether there has been any additional effect in the areas deemed most in need of free early education places. As the Government has invested in the Nursery Education Grant system to allow the funding of places in private settings in order to circumvent problems of capacity in the maintained sector, it is worth examining whether, in terms of educational outcomes, this has been a successful policy.

In terms of whether the policy has had any overall effect, the policy evaluation regressions revealed that it is only in the poorer LEAs that there are identifiable policy effects – so the areas that the Government targeted ***were*** affected by the policy's introduction.

Though the policy increased provision for 3-year olds in the non-schools sector – comprised largely of private sector places – there were no measured robust effects of this increase on results in reading or writing either overall or for either the more or less deprived LEAs when estimated separately. There is slight evidence that increased use of the private sector for 3-year olds' early education actually had a negative effect on results in maths at the higher level for the poorer LEAs but a positive effect on the level children are expected to attain in the better off LEAs.

My main finding is that there is a small but significant and robust positive effect of take-up of free early education places in maintained nursery and primary schools, by 3-year olds in the poorer LEAs, on the percentage of children attaining level 2B or higher in KS1 reading and writing. Though quite small in magnitude (approximately one-third of a within standard deviation), these effects are comparable to reducing average KS1 class size by almost one child in the case of reading, and three-quarters of a child in the case of writing. The results suggest that switching all provision of early education for 3-year olds to maintained school settings rather than private settings would increase results in reading and writing by approximately one standard deviation in the poorer LEAs. My findings are in line with the EPPE study which suggests that children attending maintained settings for pre-school perform better on school entry and at age 7.

It is also cautiously concluded that there is a significant and robust positive effect of the take-up of free early education places in maintained nursery and primary schools, by 4-year olds in the better-off LEAs, on the percentage of children attaining level 3 or higher in KS1 reading, writing and maths. The effects are still small in absolute terms but represent increases of three-quarters of a within standard deviation (reading), half of a within standard deviation (writing) and one third of a within standard deviation (maths). Given that the majority of 4-year olds attending a free early education place in a maintained nursery or primary school are actually attending a reception or infant class in a primary school, the results here suggest that, in the better off LEAs, increasing the proportion of children who start primary school early has a positive effect on KS1 results increasing the percentage of children who exceed expected levels of attainment. However this "result" is only tentative due to concerns over an upward bias in the estimated coefficient.

In terms of educational outcomes, the magnitude of the effect of increasing provision of free places for 3-year olds is small, even in maintained settings, suggesting that improving results alone may not necessarily be a justification for such universal provision. However, there are other externalities associated with early education that need to be considered. These include behavioural and socialisation gains from early education, and the development of non-cognitive skills which then facilitate learning and allow children to make the most of the education that they receive and the natural abilities that they have been endowed with. Moreover, there are wider benefits accruing to children, parents and society as a whole, as a result of parents – particularly single parents – being aided in returning to the labour market as a result of the free early education places policy, in conjunction with other policies such as the childcare element of the working tax credit.

2.9 Full Description of Variables in the Tables

3-year olds early education (EE) take-up school (sch) sector ***(t)*** = percentage of the 3-year old population in the LEA at time t that took a free early education place in a maintained nursery school or a nursery or infant class in a maintained primary school.

3-year olds early education (EE) take-up 'other' sector ***(t)*** = percentage of the 3-year old population in the LEA at time t that took a free early education place in with an alternative maintained or private, voluntary or independent provider.

4-year olds early education (EE) take-up school (sch) sector ***(t+1)*** = percentage of the 3-year old population in the LEA at time $t+1$ that took a free early education place in a maintained nursery school or a nursery or infant class in a maintained primary school.

4-year olds early education (EE) take-up 'other' sector ***(t+1)*** = percentage of the 3-year old population in the LEA at time $t+1$ that took a free early education place in with an alternative maintained or private, voluntary or independent provider.

childcare places available: day nursery (dnp) ***(t)*** = percentage of the 3- and 4-year old population in the LEA at time t that could have a place in a day nursery. Day nurseries look after children for up to the full working day.

childcare places available: childminder (cmp) ***(t)*** = percentage of the 3- and 4-year old population in the LEA at time t that could have a place with a childminder. Childminders look after children for up to the full working day.

childcare places available: playgroup (pgp) ***(t)*** = percentage of the 3- and 4-year old population in the LEA at time t that could have a place at a playgroup. Most playgroups run five sessions a week of between 2.5 and 4 hours. If sessions are run morning and afternoon children are only to attend either or.

3-year olds early education (EE) take-up school (sch) sector 'poorer' LEAs ***(t)*** = percentage of the 3-year old population in the LEA at time t that took a free early education place in a maintained nursery school or a nursery or infant class in a maintained primary school, for the pathfinder group of LEAs to receive the NEG funding for 3-year olds.

3-year olds early education (EE) take-up school (sch) sector 'better off' LEAs ***(t)*** = percentage of the 3-year old population in the LEA at time t that took a free early education place in a maintained nursery school or a nursery or infant class in a maintained primary school, for the LEAs who were not in the pathfinder group of LEAs to receive the NEG funding for 3-year olds.

3-year olds early education (EE) take-up 'other' sector ***(t)*** = percentage of the 3-year old population in the LEA at time t that took a free early education place in with an alternative maintained or private, voluntary or independent provider, for the pathfinder group of LEAs to receive the NEG funding for 3-year olds.

3-year olds early education (EE) take-up 'other' sector ***(t)*** = percentage of the 3-year old population in the LEA at time t that took a free early education place in with an alternative maintained or private, voluntary or independent provider, for the LEAs who were not in the pathfinder group of LEAs to receive the NEG funding for 3-year olds.

4-year olds early education (EE) take-up school (sch) sector 'poorer' LEAs ***(t+1)*** = percentage of the 4-year old population in the LEA at time $t+1$ that took a free early education place in a maintained nursery school or a nursery or infant class in a maintained primary school, for the pathfinder group of LEAs to receive the NEG funding for 3-year olds.

4-year olds early education (EE) take-up school (sch) sector 'better off' LEAs *(t+1)* = percentage of the 4-year old population in the LEA at time *t+1* that took a free early education place in a maintained nursery school or a nursery or infant class in a maintained primary school, for the LEAs who were not in the pathfinder group of LEAs to receive the NEG funding for 3-year olds.

4-year olds early education (EE) take-up 'other' sector 'poorer' LEAs *(t+1)* = percentage of the 4-year old population in the LEA at time *t+1* that took a free early education place in with an alternative maintained or private, voluntary or independent provider, for the pathfinder group of LEAs to receive the NEG funding for 3-year olds.

4-year olds early education (EE) take-up 'other' sector 'better off' LEAs *(t+1)* = percentage of the 4-year old population in the LEA at time *t+1* that took a free early education place in with an alternative maintained or private, voluntary or independent provider, for the LEAs who were not in the pathfinder group of LEAs to receive the NEG funding for 3-year olds.

economic inactivity rate, working age males *(t)* = % of working age males who are economically inactive at time *t*.

manufacturing jobs rate *(t)* = % of total employed who are employed in manufacturing occupations at time *t*.

median weekly gross pay, f-t male workers *(t)* = median weekly gross pay for male full time workers, all industries/occupations at time *t*, £s.

average KS1 class size *(t+3)* = average class size for KS1 classes across the whole LEA at time *(t+3)* when the children take their KS1 assessments.

% of non-white children *(t+3)* = percentage of children in maintained primary schools in the LEA who are of non-white ethnic origin at time *(t+3)* when the children take their KS1 assessments.

2.10 Tables

Table 2.3: Summary of Dependent Variables, Main Sample

Variable		**Mean**	**S.D.**	**Min**	**Max**	**Observations**			**Within Range**	
reading L3	overall	27.00	5.32	14.00	44.00	N	=	464	Mean	4.77
	between		4.91	15.25	41.00	n	=	120	Median	5
	within		2.03	18.50	32.25	$\bar{T}$	=	3.87		
reading L2B	overall	68.83	4.99	53.00	82.00	N	=	575	Mean	4.06
	between		4.72	55.50	78.20	n	=	120	Median	4
	within		1.70	64.43	75.43	$\bar{T}$	=	4.79		
writing L3	overall	13.23	4.41	1.00	25.00	N	=	464	Mean	7.12
	between		3.24	6.50	22.00	n	=	120	Median	7
	within		2.99	2.98	18.73	$\bar{T}$	=	3.87		
writing L2B	overall	59.55	5.52	44.00	72.00	N	=	575	Mean	5.1
	between		5.13	45.75	69.60	n	=	120	Median	5
	within		2.10	53.15	66.15	$\bar{T}$	=	4.79		
maths L3	overall	26.68	6.12	11.00	43.00	N	=	464	Mean	10.06
	between		4.54	16.00	38.75	n	=	120	Median	10
	within		4.09	13.93	34.93	$\bar{T}$	=	3.87		
maths L2B	overall	73.82	4.76	58.00	86.00	N	=	575	Mean	4.58
	between		4.40	61.75	82.80	n	=	120	Median	4
	within		1.84	67.22	79.22	$\bar{T}$	=	4.79		

Notes: The within figures for minimum and maximum have the overall mean of the variable added back in to make results comparable.
subject LX = percentage of children in the LEA's maintained schools achieving level X or higher in thier KS1 *subject* assessment.
subject $\in$ {*reading, writing, maths*}

Table 2.4: Summary of Dependent Variables, Main Sample, separately by 'pathfinder' group membership

Variable		Mean	S.D.	Min	Max	Observations			Within Range	
				'pathfinder' LEAs						
reading L3	overall	23.41	4.12	14.00	34.00	N	=	217	Mean	4.54
	between		3.71	15.25	30.67	n	=	56	Median	4
	within		1.90	16.16	27.41	$\bar{T}$	=	3.88		
reading L2B	overall	65.68	4.56	53.00	76.00	N	=	269	Mean	3.88
	between		4.31	55.50	74.20	n	=	56	Median	4
	within		1.66	61.28	70.88	$\bar{T}$	=	4.80		
writing L3	overall	11.39	3.59	2.00	20.00	N	=	217	Mean	6.63
	between		2.30	7.00	17.25	n	=	56	Median	7
	within		2.76	3.89	16.89	$\bar{T}$	=	3.88		
writing L2B	overall	56.30	5.24	44.00	71.00	N	=	269	Mean	5.32
	between		4.81	45.75	66.80	n	=	56	Median	5
	within		2.19	49.90	61.50	$\bar{T}$	=	4.80		
maths L3	overall	23.50	5.34	11.00	36.00	N	=	217	Mean	10.09
	between		3.54	16.00	31.50	n	=	56	Median	10
	within		4.03	11.50	30.50	$\bar{T}$	=	3.88		
maths L2B	overall	70.79	4.49	58.00	81.00	N	=	269	Mean	5.07
	between		4.07	61.75	79.20	n	=	56	Median	5
	within		2.02	64.19	76.19	$\bar{T}$	=	4.80		
				'non-pathfinder' LEAs						
reading L3	overall	30.15	4.12	20.00	44.00	N	=	247	Mean	4.97
	between		3.52	22.75	31.00	n	=	64	Median	5
	within		2.14	21.65	35.40	$\bar{T}$	=	3.86		
reading L2B	overall	71.59	3.48	60.00	82.00	N	=	306	Mean	4.22
	between		3.04	64.40	78.20	n	=	64	Median	4
	within		1.73	67.19	78.19	$\bar{T}$	=	4.78		
writing L3	overall	14.84	4.45	1.00	25.00	N	=	247	Mean	7.55
	between		3.11	6.50	22.00	n	=	64	Median	8
	within		3.19	4.59	20.34	$\bar{T}$	=	3.86		
writing L2B	overall	62.41	3.95	49.00	72.00	N	=	306	Mean	4.91
	between		3.44	53.60	69.60	n	=	64	Median	4
	within		2.01	56.21	69.01	$\bar{T}$	=	4.78		
maths L3	overall	29.48	5.34	14.00	43.00	N	=	247	Mean	10.03
	between		3.38	23.00	38.75	n	=	64	Median	10
	within		4.15	16.73	37.73	$\bar{T}$	=	3.86		
maths L2B	overall	76.48	3.11	66.00	86.00	N	=	306	Mean	4.14
	between		2.63	68.00	82.80	n	=	64	Median	4
	within		1.66	71.08	71.08	$\bar{T}$	=	4.78		

Notes: The within figures for minimum and maximum have the overall mean of the variable added back in to make results comparable.
subject LX = percentage of children in the LEA's maintained schools achieving level X or higher in thier KS1 *subject* assessment.
subject $\in$ {*reading, writing, maths*}

Table 2.5: Level 2B or higher attainment: Main Sample, by year, separately by ‘pathfinder’ group membership

		‘Pathfinder’ LEAs						**‘Non-Pathfinder’ LEAs**							
Variable		**Obs**	**Mean**	**S.D.**	**Min**	**Max**	**Variable**	**Obs**	**Mean**	**S.D.**	**Min**	**Max**	**Difference in means**		
reading L2B	**2001**	52	65.06	4.75	53	75	**reading L2B**	59	70.47	3.33	62	78	**reading L2B**	**2001**	5.42
	2002	55	64.84	4.62	54	74		62	70.53	3.27	60	77		**2002**	5.70
	2003	54	65.02	4.44	53	74		60	71.00	3.11	61	76		**2003**	5.98
	2004	53	66.42	4.55	55	75		61	72.48	3.49	65	80		**2004**	6.06
	2005	54	68.31	4.27	60	76		63	74.60	3.14	67	81		**2005**	6.29
	2006	55	67.07	4.14	60	76		64	73.38	3.29	67	82		**2006**	6.30
writing L2B	**2001**	52	54.58	5.44	44	68	**writing L2B**	59	60.51	3.94	49	68	**writing L2B**	**2001**	5.93
	2002	55	55.29	5.23	44	66		62	61.65	3.85	50	68		**2002**	6.35
	2003	54	58.06	5.17	44	71		60	64.02	3.62	54	72		**2003**	5.96
	2004	53	57.60	4.95	46	69		61	63.36	3.92	52	71		**2004**	5.76
	2005	54	57.56	4.96	47	69		63	64.14	3.72	56	74		**2005**	6.59
	2006	55	55.98	4.76	46	65		64	62.50	3.57	56	71		**2006**	6.52
maths L2B	**2001**	52	72.35	4.00	64	81	**maths L2B**	59	77.20	2.69	67	82	**maths L2B**	**2001**	4.86
	2002	55	72.11	4.19	63	80		62	77.37	2.59	70	83		**2002**	5.26
	2003	54	69.56	4.40	58	78		60	75.47	2.82	66	81		**2003**	5.91
	2004	53	71.45	4.41	63	80		61	77.18	3.44	69	86		**2004**	5.73
	2005	54	70.26	4.65	62	82		63	76.41	3.00	68	84		**2005**	6.15
	2006	55	68.58	4.33	62	78		64	75.23	3.25	68	82		**2006**	6.65

Table 2.6: Level 3 or higher attainment: Main Sample, by year, separately by 'pathfinder' group membership

		'Pathfinder' LEAs					**'Non-Pathfinder' LEAs**						**Difference in means**		
Variable		**Obs**	**Mean**	**S.D.**	**Min**	**Max**	**Variable**	**Obs**	**Mean**	**S.D.**	**Min**	**Max**			
reading L3	**2002**	55	25.38	4.24	16	34	**reading L3**	62	32.29	3.87	22	44	**reading L3**	**2002**	6.91
	2003	54	23.48	3.98	14	31		60	30.18	3.45	22	39		**2003**	6.70
	2004	53	23.77	3.57	15	30		61	30.48	3.74	22	44		**2004**	6.70
	2005	54	22.48	4.18	14	32		63	29.46	3.71	21	39		**2005**	6.98
	2006	55	21.04	3.52	14	28		64	27.75	4.09	20	39		**2006**	6.71
writing L3	**2002**	55	7.45	2.46	2	13	**writing L3**	62	10.23	3.51	1	16	**writing L3**	**2002**	2.77
	2003	54	13.69	2.93	9	20		60	17.22	3.73	6	25		**2003**	3.53
	2004	53	13.21	2.70	7	19		61	16.67	3.39	7	24		**2004**	3.46
	2005	54	12.69	2.86	7	20		63	16.59	3.19	11	29		**2005**	3.90
	2006	55	11.31	2.37	7	18		64	15.34	3.39	9	25		**2006**	4.03
maths L3	**2002**	55	27.69	3.76	21	36	**maths L3**	62	33.13	3.30	26	43	**maths L3**	**2002**	5.44
	2003	54	25.02	4.13	16	33		60	31.48	3.68	21	40		**2003**	6.46
	2004	53	23.81	3.96	15	32		61	30.23	3.96	22	40		**2004**	6.42
	2005	54	19.07	4.02	12	27		63	25.13	3.89	18	35		**2005**	6.05
	2006	55	17.51	3.37	11	25		64	23.34	4.21	14	36		**2006**	5.83

Table 2.7: Summary of Independent Variables, Main Sample

Variable		Mean	S.D.	Min	Max	Observations			Within Range	
3-year olds early education take-up, schools sector	overall	43.09	24.17	1.02	104.26	N	=	575	Mean	6.41
	between		23.89	1.40	101.34	n	=	120	Median	4.37
	within		3.62	18.50	73.07	T-bar	=	4.79		
3-year olds early education take-up, ‘other’ sector	overall	12.89	17.52	0.00	74.53	N	=	575	Mean	35.72
	between		6.11	1.68	27.38	n	=	120	Median	35.31
	within		16.42	-14.49	66.41	T-bar	=	4.79		
4-year olds early education take-up, schools sector	overall	81.71	11.99	43.38	108.65	N	=	575	Mean	5.02
	between		11.80	49.55	101.87	n	=	120	Median	4.37
	within		2.12	73.73	92.08	T-bar	=	4.79		
4-year olds early education take-up, ‘other’ sector	overall	15.36	10.31	0.00	50.17	N	=	575	Mean	6.03
	between		9.96	0.65	44.77	n	=	120	Median	4.99
	within		2.69	3.62	27.03	T-bar	=	4.79		
childcare places available: day nursery	overall	9.20	4.11	0.00	28.00	N	=	575	Mean	6.68
	between		3.12	3.23	17.41	n	=	120	Median	6.6
	within		2.69	-0.82	20.29	T-bar	=	4.79		
childcare places available: childminder	overall	10.22	4.47	1.94	29.58	N	=	575	Mean	3.24
	between		4.16	2.39	26.65	n	=	120	Median	2.43
	within		1.56	-0.19	20.18	T-bar	=	4.79		
childcare places available: playgroup	overall	24.41	12.07	3.59	62.98	N	=	575	Mean	8.45
	between		11.41	6.53	52.81	n	=	120	Median	6.84
	within		3.95	2.22	39.72	T-bar	=	4.79		

Notes: The within figures for minimum and maximum have the overall mean of the variable added back in to make results comparable.
See Full Description of Variables in the Tables, page 66, for definition of each variable.

Table 2.8: Summary of Independent Variables, Main Sample, separately by 'pathfinder' group membership

'pathfinder' LEAs

Variable		Mean	S.D.	Min	Max	Within Range	
3-year olds EE take-up, schools sector	overall	59.39	16.70	8.83	104.26	Mean	6.96
	between		16.55	9.38	96.03	Median	5.06
	within		3.25	34.79	76.76		
3-year olds EE take-up, 'other' sector	overall	13.85	15.01	0.00	69.82	Mean	28.79
	between		6.66	2.39	27.38	Median	30.66
	within		13.45	-13.54	56.29		
4-year olds EE take-up, schools sector	overall	88.08	9.22	60.35	108.65	Mean	5.37
	between		9.02	63.53	101.84	Median	4.89
	within		2.23	81.62	98.45		
4-year olds EE take-up, 'other' sector	overall	9.69	7.68	0.00	4.42	Mean	5.79
	between		7.35	0.99	31.92	Median	4.79
	within		2.52	2.31	21.36		
childcare places available: day nursery	overall	8.83	4.12	0.65	28.00	Mean	6.12
	between		3.20	3.23	17.41	Median	5.56
	within		2.59	-1.19	19.92		
childcare places available: childminder	overall	8.04	3.19	1.94	20.29	Mean	2.74
	between		2.93	2.79	15.16	Median	1.99
	within		1.30	2.40	18.01		
childcare places available: playgroup	overall	16.65	8.72	3.92	58.99	Mean	6.84
	between		8.43	6.53	52.35	Median	5.87
	within		3.08	3.95	30.66		
Observations:	N = 269		n = 56		T = 4.80		

'non-pathfinder' LEAs

Variable		Mean	S.D.	Min	Max	Within Range	
3-year olds EE take-up, schools sector	overall	28.77	20.34	1.02	104.02	Mean	5.92
	between		20.41	1.40	101.34	Median	3.80
	within		3.93	6.43	58.75		
3-year olds EE take-up, 'other' sector	overall	12.05	19.44	0.00	74.53	Mean	41.79
	between		5.49	1.68	25.34	Median	43.71
	within		18.66	-13.29	65.57		
4-year olds EE take-up, schools sector	overall	76.10	11.33	43.38	104.86	Mean	4.71
	between		11.28	49.55	101.87	Median	4.20
	within		2.02	68.12	82.24		
4-year olds EE take-up, 'other' sector	overall	20.36	9.75	0.00	50.17	Mean	6.24
	between		9.41	0.65	44.77	Median	5.20
	within		2.83	8.61	31.21		
childcare places available: day nursery	overall	9.52	4.07	0.00	22.22	Mean	7.17
	between		3.03	3.31	15.25	Median	6.95
	within		2.78	1.79	17.19		
childcare places available: childminder	overall	12.13	4.55	1.98	29.58	Mean	3.68
	between		4.17	2.39	26.65	Median	2.82
	within		1.75	1.73	19.55		
childcare places available: playgroup	overall	31.22	10.38	3.59	62.98	Mean	9.86
	between		9.29	13.04	52.81	Median	8.73
	within		4.58	9.04	46.54		
Observations:	N = 306		n = 64		T = 4.80		

Notes: The within figures for minimum and maximum have the overall mean of the variable added back in to make results comparable. See page 66 for Full Description of Variables in the Tables.

Table 2.9: Summary of Additional Independent Variables, Main Sample

Variable		**Mean**	**S.D.**	**Min**	**Max**	**Observations**		**Within Range**	
economic in activity rate,	overall	16.14	5.55	0.00	40.10	N	575	Mean	7.12
working age males	between		4.74	6.11	29.33	n	120	Median	6.64
	within		2.93	6.68	26.90	T-bar	4.79		
manfacturing jobs rate	overall	16.54	6.70	1.81	41.01	N	575	Mean	6.02
	between		6.22	3.53	33.26	n	120	Median	5.48
	within		2.50	5.14	29.90	T-bar	4.79		
median weekly gross pay,	overall	405.11	63.13	266.10	679.00	N	575	Mean	72.53
full-time male workers, £s	between		56.23	303.06	610.48	n	120	Median	70.75
	within		28.57	306.15	519.55	T-bar	4.79		
average KS1 class size	overall	25.64	1.15	22.90	28.30	N	575	Mean	1.38
	between		1.01	23.66	27.88	n	120	Median	1.30
	within		0.55	24.06	28.10	T-bar	4.79		
% of non-white children	overall	18.51	21.36	0.00	80.91	N	574	Mean	4.81
	between		21.16	0.94	77.47	n	120	Median	3.79
	within		2.22	8.93	29.43	T-bar	4.78		

Notes: The within figures for minimum and maximum have the overall mean of the variable added back in to make results comparable.
See Full Description of Variables in the Tables, page 66, for definition of each variable.

Table 2.10: Summary of Additional Independent Variables, Main Sample, separately by 'pathfinder' group membership

Variable		Mean	S.D.	Min	Max	Within Range	
		'pathfinder' LEAs					
economic inactivity rate, working age males	overall	19.66	4.81	8.58	40.10	Mean	8.42
	between		3.51	12.77	29.33	Median	8.12
	within		3.31	10.20	30.42		
manfacturing jobs rate	overall	16.79	7.50	3.10	41.01	Mean	6.37
	between		7.06	6.78	33.26	Median	6.28
	within		2.53	8.61	24.54		
median weekly gross pay, full-time male workers, £s	overall	406.89	72.02	283.40	679.00	Mean	69.38
	between		66.20	313.62	610.48	Median	62.65
	within		28.40	307.93	521.33		
average KS1 class size	overall	25.65	1.13	22.90	28.30	Mean	1.36
	between		1.01	23.66	27.88	Median	1.30
	within		0.53	24.53	27.35		
% of non-white children	overall	27.38	25.05	0.78	80.91	Mean	5.64
	between		25.07	1.25	77.47	Median	5.53
	within		2.50	19.74	38.30		
Observations:	N = 269		n = 56		$\bar{T}$ = 4.80		
		'non-pathfinder' LEAs					
economic inactivity rate, working age males	overall	13.05	4.15	0.00	35.49	Mean	5.99
	between		3.38	6.11	24.96	Median	5.61
	within		2.56	4.94	23.58		
manfacturing jobs rate	overall	16.32	5.92	1.81	32.49	Mean	5.71
	between		5.42	3.53	27.37	Median	5.08
	within		2.48	4.92	29.68		
median weekly gross pay, full-time male workers, £s	overall	403.54	54.20	266.10	577.10	Mean	75.30
	between		46.31	303.06	526.82	Median	76.45
	within		28.77	344.72	480.82		
average KS1 class size	overall	25.63	1.16	22.90	28.00	Mean	1.40
	between		1.03	23.66	27.40	Median	1.30
	within		0.57	24.05	28.09		
% of non-white children	overall	10.75	13.36	0.00	64.69	Mean	4.08
	between		13.12	0.94	58.64	Median	3.11
	within		1.95	1.16	17.96		
Observations:	N = 306		n = 64		$\bar{T}$ = 4.78		

Notes: The within figures for minimum and maximum have the overall mean of the variable added back in to make results comparable. See page 66 for Full Description of Variables in the Tables.

Table 2.11: The Effect of Free Early Education Place Policy on **Reading**

Dependent Variable: Percentage of children in the LEA's maintained school achieving the specified level in Key Stage 1 assessment, year (t+3)

Panel Regression Models: 5 year panel (Level 2B or higher), 4 year panel (Level 3 or higher)

	Level 2B or higher		**Level 3 or higher**	
Independent Variable	#1	#2	#1	#2
policy (t)	-0.007		0.184	
	0.276		0.235	
policy 'poorer' LEAs (t)		-0.156		**0.425**†
		0.320		0.266
policy 'better-off' LEAs (t)		0.138		0.062
		0.336		0.301
year=2002	-0.128	-0.129		
	0.162	0.162		
year=2003	0.119	0.189	**-2.208*****	**-2.322*****
	0.239	0.228	0.174	0.182
year=2004	**1.791*****	**1.783*****	**-1.784*****	**-1.833*****
	0.317	0.319	0.287	0.277
year=2006	**2.471*****	**2.463*****	**-4.587*****	**-4.635*****
	0.354	0.355	0.317	0.312
constant	**67.969*****	**67.969*****	**29.046*****	**29.046*****
	0.140	0.140	0.112	0.112
R^2 within	0.397	0.398	0.61	0.611
R^2 between	0.000	0.109	0.013	0.233
R^2 overall	0.045	0.065	0.083	0.058
ρ	0.91	0.908	0.917	0.919
#obs	575	575	464	464
#groups	120	120	120	120
LEA level fixed effects included	Yes	Yes	Yes	Yes

Robust Standard Errors, lower figure; ρ is the fraction of the variance due to the fixed effects

* p<0.10, ** p<0.05, *** p<0.01; † p=0.112

Notes: Model #1: common policy effect

Model #2: allowing for different effects of the policy in the 'poorer' and 'better-off' LEAs.

Interpretation of the coefficients: The dependent variable is the percentage of children in an LEA's maintained schools attaining the specified level in KS1 reading. The estimated coefficient on the policy dummy in the pathfinder LEAs for level 3 or higher of 0.425 suggests that the introduction of the policy in these LEAs is associated with a 0.425%-point increase in the percentage of childrenin the LEA attaining level 3 or higher in reading at KS1.

Table 2.12: The Effect of Free Early Education Place Policy on **Writing**

Dependent Variable: Percentage of children in the LEA's maintained school achieving the specified level in Key Stage 1 assessment, year (t+3)

Panel Regression Models: 5 year panel (Level 2B or higher), 4 year panel (Level 3 or higher)

	Level 2B or higher		**Level 3 or higher**	
Independent Variable	#1	#2	#1	#2
policy (t)	0.255		-0.057	
	0.362		0.325	
policy 'poorer' LEAs (t)		0.422		**-0.728**†
		0.447		0.448
policy 'better-off' LEAs (t)		0.093		0.282
		0.413		0.342
year=2002	**0.885*****	**0.885*****		
	0.212	0.212		
year=2003	**3.221*****	**3.143*****	**6.625*****	**6.941*****
	0.310	0.315	0.292	0.335
year=2004	**2.804*****	**2.813*****	**6.268*****	**6.403*****
	0.407	0.406	0.396	0.406
year=2006	**1.454*****	**1.463*****	**4.612*****	**4.746*****
	0.459	0.460	0.413	0.423
constant	**57.752*****	**57.752*****	**8.911*****	**8.909*****
	0.166	0.166	0.155	0.152
R^2 within	0.355	0.356	0.76	0.764
R^2 between	0.001	0.069	0.007	0.187
R^2 overall	0.047	0.032	0.353	0.410
ρ	0.88	0.882	0.78	0.765
#obs	575	575	464	464
#groups	120	120	120	120
LEA level fixed effects included	Yes	Yes	Yes	Yes

Robust Standard Errors, lower figure ρ is the fraction of the variance due to the fixed effects

* p<0.10, ** p<0.05, *** p<0.01; † p=0.107

Notes: Model #1: common policy effect

Model #2: allowing for different effects of the policy in the 'poorer' and 'better-off' LEAs.

Interpretation of the coefficients: The dependent variable is the percentage of children in an LEA's maintained schools attaining the specified level in KS1 reading. The estimated coefficient on the policy dummy in the pathfinder LEAs for level 3 or higher of -0.728 suggests that the introduction of the policy in these LEAs is associated with a 0.728%-point decrease in the percentage of childrenin the LEA attaining level 3 or higher in writing at KS1.

Table 2.13: The Effect of Free Early Education Place Policy on **Maths**

Dependent Variable: Percentage of children in the LEA's maintained school achieving the specified level in Key Stage 1 assessment, year (t+3)

Panel Regression Models: 5 year panel (Level 2B or higher), 4 year panel (Level 3 or higher)

	Level 2B or higher		**Level 3 or higher**	
Independent Variable	#1	#2	#1	#2
policy (t)	-0.250		-0.469	
	0.285		0.346	
policy 'poorer' LEAs (t)		**-0.722****		**-0.825****
		0.331		0.407
policy 'better-off' LEAs (t)		0.205		-0.289
		0.330		0.389
year=2002	-0.11	-0.111		
	0.159	0.159		
year=2003	**-2.265*****	**-2.045*****	**-2.002*****	**-1.834*****
	0.213	0.224	0.251	0.290
year=2004	-0.149	-0.173	**-2.803*****	**-2.731*****
	0.350	0.346	0.422	0.425
year=2006	**-2.605*****	**-2.630*****	**-9.516*****	**-9.445*****
	0.372	0.365	0.447	0.452
constant	**74.984*****	**74.985*****	**30.592*****	**30.591*****
	0.131	0.128	0.138	0.137
R^2 within	0.438	0.451	0.841	0.842
R^2 between	0.002	0.284	0.016	0.185
R^2 overall	0.068	0.138	0.384	0.412
ρ	0.889	0.884	0.849	0.842
#obs	575	575	464	464
#groups	120	120	120	120
LEA level fixed effects included	Yes	Yes	Yes	Yes
Robust Standard Errors, lower figure * p<0.10, ** p<0.05, *** p<0.01	ρ is the fraction of the variance due to the fixed effects			

Notes: Model #1: common policy effect
Model #2: allowing for different effects of the policy in the 'poorer' and 'better-off' LEAs.
Interpretation of the coefficients: The dependent variable is the percentage of children in an LEA's maintained schools attaining the specified level in KS1 reading. The estimated coefficient on the policy dummy in the pathfinder LEAs for level 3 or higher of -0.825 suggests that the introduction of the policy in these LEAs is associated with a 0.825%-point increase in the percentage of childrenin the LEA attaining level 3 or higher in maths at KS1.

Table 2.14: The Effect of Free Early Education Places on **Reading**, Model #1

Dependent Variable: Percentage of children in the LEA's maintained school achieving the specified level in Key Stage 1 assessment, year (*t+3*)

Panel Regression Models: 5 year panel (Level 2B or higher), 4 year panel (Level 3 or higher)

	Level 2B or higher		**Level 3 or higher**	
Independent Variable	Coeff.	Rob. St. Err.	Coeff.	Rob. St. Err.
3-year olds early educ take-up, school sector (t)	**0.037****	0.017	-0.008	0.031
3-year olds early educ take-up, 'other' sector (t)	0.000	0.008	0.006	0.009
4-year olds early educ take-up, school sector (t+1)	-0.013	0.046	0.055	0.043
4-year olds early educ olds take-up, 'other' sector (t+1)	-0.001	0.023	0.023	0.026
childcare places available: day nursery (t)	0.015	0.037	0.010	0.039
childcare places available: childminder (t)	0.052	0.049	-0.035	0.043
childcare places available: playgroup (t)	-0.011	0.017	-0.014	0.020
average KS1 class size (t+3)	**-0.651*****	0.161	**-0.765*****	0.169
year=2002	-0.168	0.163		
year=2003	0.230	0.213	**-1.959*****	0.158
year=2004	**1.969*****	0.267	**-1.444*****	0.253
year=2006	**2.895*****	0.458	**-4.062*****	0.426
constant	**83.634*****	5.509	**44.596*****	5.930
R^2 within	0.432		0.641	
R^2 between	0.007		0.011	
R^2 overall	0.012		0.049	
ρ	0.919		0.926	
#obs	575		464	
#groups	120		120	
LEA level fixed effects included	Yes		Yes	

ρ is the fraction of the variance due to the fixed effects

* p<0.10, ** p<0.05, *** p<0.01

Notes: Interpretation of the coefficients: The key independent variables, 3-year olds and 4-year olds take-up rate of free early education places in school and 'other' sector settings, are take-up rates expressed as percentages. The dependent variable is the percentage of children in an LEA's maintained schools attaining the specified level in KS1 reading. The estimated coefficient on 3-year olds' take-up of free early education places in maintained schools of 0.037 suggests that a 10%-point increase in the percentage of 3-year olds taking a free place in a maintained school is associated with a 0.37%-point increase in the percentage of children in the LEA attaining level 2B or higher in reading at KS1.

See Full Description of Variables in the Tables, page 66, for definition of each variable.

Table 2.15: The Effect of Free Early Education Places on **Writing**, Model #1

Dependent Variable: Percentage of children in the LEA's maintained school achieving the specified level in Key Stage 1 assessment, year (*t+3*)

Panel Regression Models: 5 year panel (Level 2B or higher), 4 year panel (Level 3 or higher)

	Level 2B or higher		**Level 3 or higher**	
Independent Variable	Coeff.	Rob. St. Err.	Coeff.	Rob. St. Err.
3-year olds early educ take-up, school sector (t)	0.011	0.019	0.012	0.024
3-year olds early educ take-up, 'other' sector (t)	0.010	0.010	0.010	0.009
4-year olds early educ take-up, school sector ($t+1$)	-0.017	0.048	0.047	0.054
4-year olds early educ olds take-up, 'other' sector ($t+1$)	0.012	0.029	-0.030	0.030
childcare places available: day nursery (t)	0.010	0.045	0.055	0.044
childcare places available: childminder (t)	-0.072	0.058	-0.037	0.063
childcare places available: playgroup (t)	-0.005	0.019	-0.036	0.023
average KS1 class size ($t+3$)	**-0.943*** **	0.217	**-0.456** **	0.183
year=2002	**0.830*** **	0.229		
year=2003	**3.413*** **	0.316	**6.542*** **	0.273
year=2004	**3.090*** **	0.371	**6.069*** **	0.333
year=2006	**2.040*** **	0.631	**4.218*** **	0.504
constant	**83.215*** **	6.441	**17.426** **	6.977
R^2 within	0.398		0.770	
R^2 between	0.003		0.051	
R^2 overall	0.052		0.187	
ρ	0.886		0.838	
#obs	575		464	
#groups	120		120	
LEA level fixed effects included	Yes		Yes	

ρ is the fraction of the variance due to the fixed effects

* p<0.10, ** p<0.05, *** p<0.01

Notes: Interpretation of the coefficients: The key independent variables, 3-year olds and 4-year olds take-up rate of free early education places in school and 'other' sector settings, are take-up rates expressed as percentages. The dependent variable is the percentage of children in an LEA's maintained schools attaining the specified level in KS1 reading. The estimated coefficient on 3-year olds' take-up of free early education places in the 'other' sector of 0.010 suggests that a 10%-point increase in the percentage of 3-year olds taking a free place in the 'other' sector is associated with a 0.10%-point increase in the percentage of children in the LEA attaining level 2B or higher in writing at KS1 (though it is not a statistically significant result).

See Full Description of Variables in the Tables, page 66, for definition of each variable.

Table 2.16: The Effect of Free Early Education Places on **Maths**, Model #1

Dependent Variable: Percentage of children in the LEA's maintained school achieving the specified level in Key Stage 1 assessment, year (*t+3*)

Panel Regression Models: 5 year panel (Level 2B or higher), 4 year panel (Level 3 or higher)

	Level 2B or higher		**Level 3 or higher**	
Independent Variable	Coeff.	Rob. St. Err.	Coeff.	Rob. St. Err.
3-year olds early educ take-up, school sector (*t*)	-0.013	0.024	-0.018	0.032
3-year olds early educ take-up, 'other' sector (*t*)	0.010	0.009	-0.004	0.012
4-year olds early educ take-up, school sector (*t+1*)	0.010	0.039	0.070	0.047
4-year olds early educ olds take-up, 'other' sector (*t+1*)	-0.025	0.026	-0.018	0.029
childcare places available: day nursery (*t*)	-0.029	0.038	0.027	0.05
childcare places available: childminder (*t*)	0.054	0.049	0.034	0.069
childcare places available: playgroup (*t*)	-0.020	0.017	-0.002	0.028
average KS1 class size (*t+3*)	**-0.348****	0.166	**-0.697*****	0.168
% of non-white children (*t+3*)	**-0.129*****	0.046	-0.005	0.085
year=2002	0.037	0.178		
year=2003	**-1.964*****	0.226	**-1.977*****	0.289
year=2004	0.093	0.317	**-2.886*****	0.425
year=2006	**-2.075*****	0.498	**-9.282*****	0.641
constant	**86.098*****	4.907	**43.138*****	6.673
R^2 within	0.459		0.846	
R^2 between	0.425		0.012	
R^2 overall	0.433		0.377	
ρ	0.825		0.852	
#obs	574		463	
#groups	120		120	
LEA level fixed effects included	Yes		Yes	

ρ is the fraction of the variance due to the fixed effects

* p<0.10, ** p<0.05, *** p<0.01

Notes: Interpretation of the coefficients: The key independent variables, 3-year olds and 4-year olds take-up rate of free early education places in school and 'other' sector settings, are take-up rates expressed as percentages. The dependent variable is the percentage of children in an LEA's maintained schools attaining the specified level in KS1 reading. The estimated coefficient on 3-year olds' take-up of free early education places in the 'other' sector of 0.010 suggests that a 10%-point increase in the percentage of 3-year olds taking a free place in the 'other' sector is associated with a 0.10%-point increase in the percentage of children in the LEA attaining level 2B or higher in maths at KS1 (though it is not a statistically significant result).

See Full Description of Variables in the Tables, page 66, for definition of each variable.

Table 2.17: The Effect of Free Early Education Places on **Reading**, Model #2

Dependent Variable: Percentage of children in the LEA's maintained school achieving the specified level in Key Stage 1 assessment, year (*t+3*)

Panel Regression Models: 5 year panel (Level 2B or higher), 4 year panel (Level 3 or higher)

	Level 2B or higher		**Level 3 or higher**	
Independent Variable	Coeff.	Rob. St. Err	Coeff.	Rob. St. Err
3-year olds EE take-up, sch sector 'poorer' LEAs (t)	**0.059****	0.024	0.033	0.043
3-year olds EE take-up, sch sector 'better-off' LEAs (t)	0.037	0.023	-0.024	0.037
3-year olds EE take-up, 'other' sector 'poorer' LEAs (t)	-0.013	0.015	0.000	0.012
3-year olds EE take-up, 'other' sector 'better-off' LEAs (t)	0.003	0.007	0.003	0.010
4-year olds EE take-up, sch sector 'poorer' LEAs ($t+1$)	-0.005	0.058	-0.055	0.050
4-year olds EE take-up, sch sector 'better-off' LEAs ($t+1$)	-0.021	0.068	**0.159****	0.069
4-year olds EE take-up, 'other' sector 'poorer' LEAs ($t+1$)	0.027	0.038	0.047	0.033
4-year olds EE take-up, 'other' sector 'better-off' LEAs ($t+1$)	0.000	0.028	0.011	0.040
childcare places available: day nursery (t)	0.014	0.036	-0.012	0.040
childcare places available: childminder (t)	0.060	0.052	-0.02	0.043
childcare places available: playgroup (t)	-0.012	0.016	-0.015	0.020
economic inactivity rate, working age males (t)	**-0.057****	0.026	-0.031	0.028
manufacturing jobs rate (t)	**-0.046***	0.027		
median weekly gross pay, male f-t workers (t)			**0.009***	0.005
average KS1 class size ($t+3$)	**-0.668*****	0.164	**-0.779*****	0.166
year=2002	-0.222	0.171		
year=2003	0.248	0.236	**-2.007*****	0.190
year=2004	**1.935*****	0.315	**-1.557*****	0.274
year=2006	**2.843*****	0.524	**-4.220*****	0.510
constant	**85.024*****	5.254	**41.508*****	6.189
R^2 within	0.447		0.655	
R^2 between	0.189		0.453	
R^2 overall	0.053		0.462	
ρ	0.941		0.932	
#obs	575		464	
#groups	120		120	
LEA level fixed effects included	Yes		Yes	

ρ is the fraction of the variance due to the fixed effects; * p<0.10, ** p<0.05, *** p<0.01

Notes: Interpretation of the coefficients: The key independent variables are take-up rates expressed as percentages. The dep. var is the % of children in an LEA's maintained schools attaining the specified level in KS1 reading. The estimated coefficient on 'poorer' LEAs' 3-year olds' take-up of free EE places in maintained schools of 0.059 suggests that a 10%-point increase in the % of 3-year olds taking a free place in a school is associated with a 0.59%-point increase in the % of children in the LEA attaining L2B+ in KS1 reading.

Table 2.18: The Effect of Free Early Education Places on **Writing**, Model #2

Dependent Variable: Percentage of children in the LEA's maintained school achieving the specified level in Key Stage 1 assessment, year (*t+3*)

Panel Regression Models: 5 year panel (Level 2B or higher), 4 year panel (Level 3 or higher)

	Level 2B or higher		**Level 3 or higher**	
Independent Variable	Coeff.	Rob. St. Err.	Coeff.	Rob. St. Err.
3-year olds EE take-up, sch sector 'poorer' LEAs (t)	**0.070****	0.031	0.016	0.039
3-year olds EE take-up, sch sector 'better-off' LEAs (t)	-0.012	0.020	-0.011	0.028
3-year olds EE take-up, 'other' sector 'poorer' LEAs (t)	-0.005	0.017	-0.016	0.015
3-year olds EE take-up, 'other' sector 'better-off' LEAs (t)	0.012	0.011	0.011	0.009
4-year olds EE take-up, sch sector 'poorer' LEAs ($t+1$)	-0.056	0.061	-0.008	0.069
4-year olds EE take-up, sch sector 'better-off' LEAs ($t+1$)	0.009	0.073	**0.157****	0.077
4-year olds EE take-up, 'other' sector 'poorer' LEAs ($t+1$)	0.029	0.057	-0.020	0.045
4-year olds EE take-up, 'other' sector 'better-off' LEAs ($t+1$)	0.020	0.033	-0.001	0.039
childcare places available: day nursery (t)	0.008	0.045	0.029	0.044
childcare places available: childminder (t)	-0.060	0.060	-0.034	0.063
childcare places available: playgroup (t)	-0.009	0.019	**-0.039***	0.023
economic inactivity rate, working age males (t)	-0.047	0.029		
manufacturing jobs rate (t)	-0.061	0.039	**0.082****	0.041
average KS1 class size ($t+3$)	**-0.948*****	0.228	**-0.450****	0.174
year=2002	**0.754*****	0.230		
year=2003	**3.433*****	0.350	**6.890*****	0.330
year=2004	**3.084*****	0.426	**6.582*****	0.406
year=2006	**2.048*****	0.717	**4.927*****	0.598
constant	**84.247*****	6.243	**13.768***	7.005
R^2 within	0.410		0.779	
R^2 between	0.162		0.321	
R^2 overall	0.189		0.396	
ρ	0.869		0.894	
#obs	575		464	
#groups	120		120	
LEA level fixed effects included	Yes		Yes	

ρ is the fraction of the variance due to the fixed effects; * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Notes: Interpretation of the coefficients: see Table 2.17

Table 2.19: The Effect of Free Early Education Places on **Maths**, Model #2

Dependent Variable: Percentage of children in the LEA's maintained school achieving the specified level in Key Stage 1 assessment, year (*t+3*)

Panel Regression Models: 5 year panel (Level 2B or higher), 4 year panel (Level 3 or higher)

	Level 2B or higher		**Level 3 or higher**	
Independent Variable	Coeff.	Rob. St. Err.	Coeff.	Rob. St. Err.
3-year olds EE take-up, sch sector 'poorer' LEAs (*t*)	-0.021	0.037	0.016	0.043
3-year olds EE take-up, sch sector 'better-off' LEAs (*t*)	-0.014	0.034	-0.051	0.039
3-year olds EE take-up, 'other' sector 'poorer' LEAs (*t*)	-0.020	0.014	**-0.025**†	0.015
3-year olds EE take-up, 'other' sector 'better-off' LEAs (*t*)	**0.018****	0.008	-0.003	0.013
4-year olds EE take-up, sch sector 'poorer' LEAs (*t+1*)	-0.022	0.054	-0.014	0.061
4-year olds EE take-up, sch sector 'better-off' LEAs (*t+1*)	0.060	0.049	**0.155****	0.072
4-year olds EE take-up, 'other' sector 'poorer' LEAs (*t+1*)	0.056	0.040	0.029	0.041
4-year olds EE take-up, 'other' sector 'better-off' LEAs (*t+1*)	-0.028	0.029	-0.020	0.047
childcare places available: day nursery (*t*)	-0.033	0.038	0.010	0.050
childcare places available: childminder (*t*)	0.067	0.047	0.050	0.069
childcare places available: playgroup (*t*)	-0.022	0.018	-0.007	0.029
economic inactivity rate, working age males (*t*)	-0.010	0.023	0.028	0.036
manufacturing jobs rate (*t*)	**-0.052***	0.029		
median weekly gross pay, male f-t workers (*t*)			0.005	0.006
average KS1 class size (*t+3*)	**-0.425****	0.164	**-0.726*****	0.170
% of non-white children (*t+3*)	**-0.120*****	0.045	0.005	0.086
year=2002	0.041	0.186		
year=2003	**-1.766*****	0.236	**-1.869*****	0.307
year=2004	0.200	0.340	**-2.819*****	0.432
year=2006	**-2.023*****	0.535	**-9.270*****	0.671
constant	**88.156*****	4.680	**40.709*****	6.550
R^2 within	0.482		0.850	
R^2 between	0.627		0.427	
R^2 overall	0.599		0.575	
ρ	0.809		0.825	
#obs	574		463	
#groups	120		120	
LEA level fixed effects included	Yes		Yes	
ρ is the fraction of the variance due to the fixed effects; * p<0.10, ** p<0.05, *** p<0.01; † p=0.102				

Notes: Interpretation of the coefficients: see Table 2.17

Table 2.20: The Percentage of 4-year olds in nursery and primary school who are in an infant or reception class in a primary school, by Government Office Region and Year*

	1998	1999	2000	2002
ENGLAND	70.41	71.31	72.23	75.01
NORTH EAST	69.51	68.73	69.79	70.65
NORTH WEST	75.16	74.92	75.35	76.93
YORKSHIRE AND THE HUMBER	61.14	61.99	62.11	67.74
EAST MIDLANDS	59.01	60.98	66.08	69.42
WEST MIDLANDS	73.88	73.99	73.91	75.09
EAST OF ENGLAND	66.33	67.98	69.82	75.20
LONDON	61.47	64.29	65.53	68.20
INNER LONDON	60.12	66.09	67.35	68.15
OUTER LONDON	62.28	63.21	64.33	68.23
SOUTH EAST	79.26	79.65	80.19	82.87
SOUTH WEST	88.75	89.37	89.25	89.97

* Data unavailable for 2001 and 2004

2.11 Figures

Figure 2.1: Kernel Density Plots of the Distributions of Test Results, Pathfinder versus Non-Pathfinder LEAs

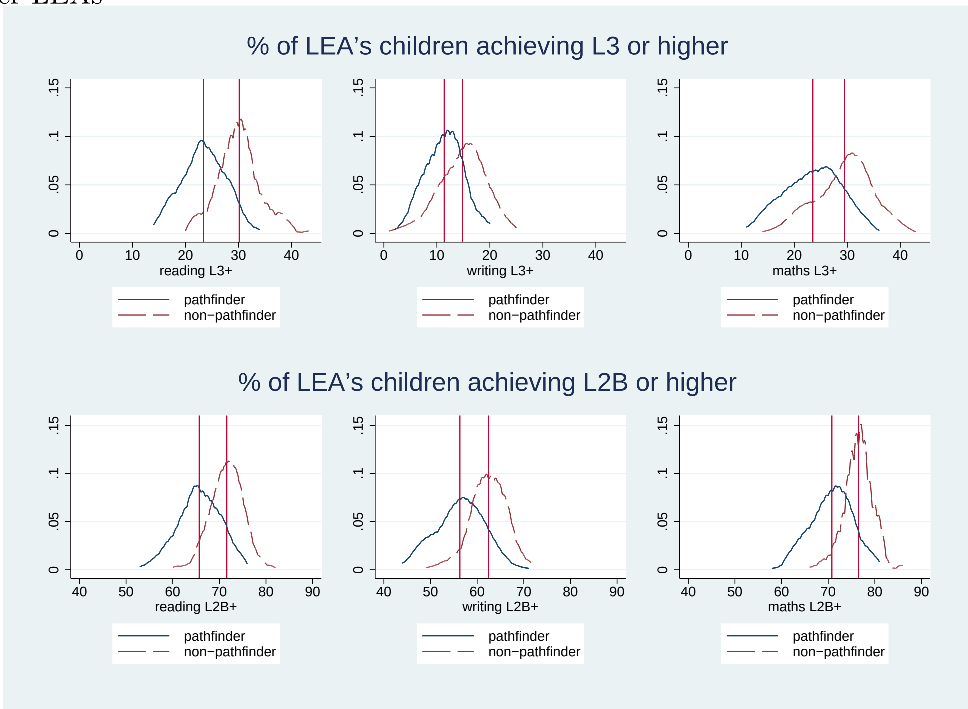


Figure 2.2: Differences in Attainment: how much higher are the Non-Pathfinder LEAs' Results compared with the Pathfinders?

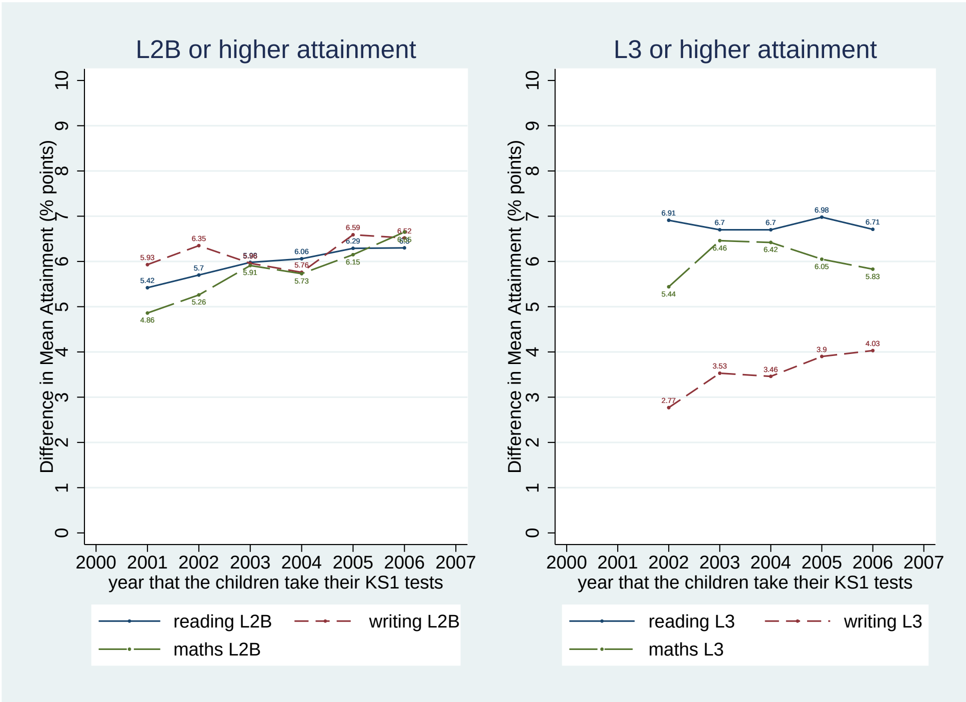


Figure 2.3: Percentiles of the Take-Up Rate of Free Early Education Places by 3-year olds in Maintained Nursery and Primary Schools

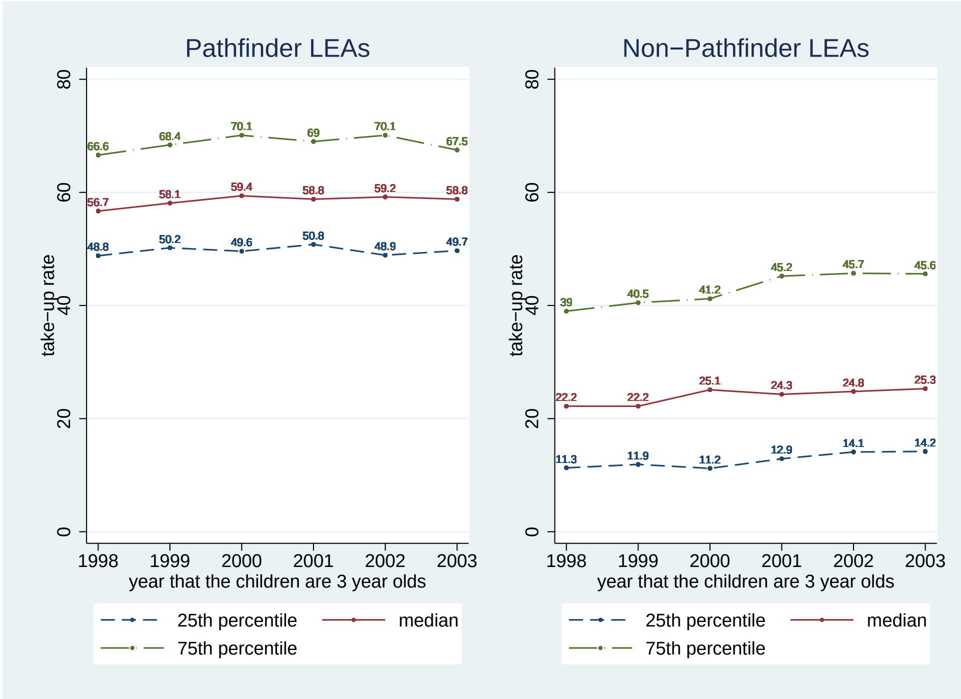
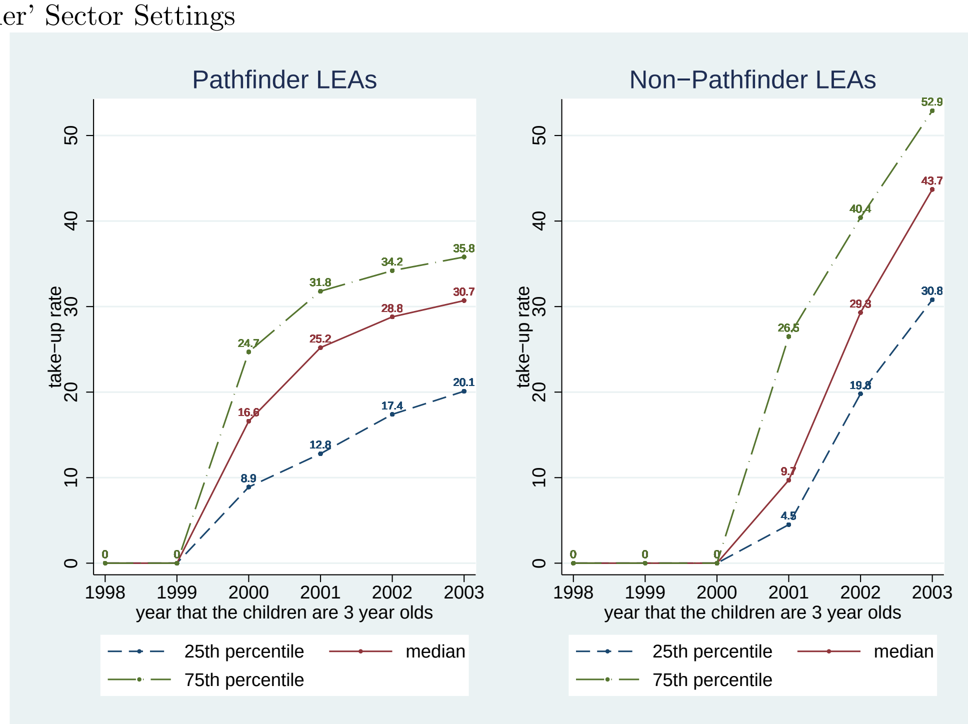


Figure 2.4: Percentiles of the Take-Up Rate of Free Early Education Places by 3-year olds in the 'Other' Sector Settings



Chapter 3

The Causal Effect of Education on Wages Revisited

3.1 Introduction

This paper estimates the causal impact of education on wages using two alternative methods of instrumentation. I compare estimates that are derived using variations in schooling associated with early smoking behaviour, with estimates derived by exploiting the impact on schooling of the raising of the minimum school leaving age. The latter instrument follows in the tradition of Card (1995) and similar papers[1], which use institutional factors or elements of the budget constraint to create instruments. This earlier research using instrumental variable methods covers a wide range and my work here is motivated by the worry that these instrumental variable methods identify a 'local average treatment effect' which might be rather different to the average effect on the treated and that will differ across instruments. These IV estimates isolate the return to education for the group whose education decision is most affected by the institutional feature exploited or the change in their own budget constraint, which may be quite a specific and unrepresentative group. The raising of the minimum school leaving age affected only those who that had wanted to leave school early and therefore, in this case, IV estimates the effects of additional schooling for those at the bottom of the schooling distribution who were forced to stay longer. In contrast, I find that early smoking affects the schooling decisions of individuals

[1]The first notable paper to use instrumental variables to estimate the return to education was Angrist and Krueger (1991). A UK study by Harmon and Walker (1995 *inter alia*) also exploited the minimum school leaving age change.

across the whole of the distribution – that is, it is not only individuals at a certain point in the schooling distribution who are affected. I interpret the estimates from this latter exercise as closer to an average effect of additional schooling akin to least squares but corrected for endogeneity. My contribution is to investigate the extent to which this effect differs from the local effect at the bottom of the distribution, implementing the alternative instrumental variables strategies using the same data from the British Household Panel Survey. In addition, as I have multiple instruments I am able to test the validity of the exclusion restrictions, something that is rarely possible to do, and also to simultaneously exploit two differing sources of exogenous variation in order to derive a further estimate of the return to education. The next section introduces the problem of estimating the return to education, section 3.3 then discusses potential solutions. Section 3.4 proposes early smoking as an instrument for education, before Section 3.5 describes the data. Section 3.6 explains the estimation procedure, section 3.7 the results and section 3.8 analyses these results and considers various tests of the instrument. Section 3.9 then compares the smoking instrument estimates with ones derived from the raising of the school leaving age, and then section 3.10 exploits the presence of two instruments to formally test the validity of these instruments. Section 3.11 offers some concluding remarks.

3.2 The Problem of Estimating the Return to Education

The foundation of the education returns literature has been Mincer's (1974) human capital earnings function:

$$\ln w_i = X_i'\varphi + \beta S_i + \epsilon_i \tag{3.1}$$

in which w_i is the wage, X_i is a vector of the individual's characteristics, including experience and experience-squared, and S_i is the number of years of schooling, determined by:

$$S_i = X_i'\gamma + u_i \tag{3.2}$$

This human capital earnings function tells us the expected (log) wage that an individual will earn given his/her observable characteristics and years of education. It is well known that if this relationship in equation (3.1) is estimated by least squares the estimate of the parameter

β can only be interpretable as the causal effect on wages of one additional year of schooling if $\mathrm{E}(X_i\ \epsilon_i) = 0$ and $\mathrm{E}(S_i\ \epsilon_i) = 0$. If however $\mathrm{E}(S_i\ \epsilon_i) \neq 0$, though we can still interpret the equation as the conditional expectation of $\ln w_i$ given X_i and S_i, we cannot interpret β as the *causal* effect of education on wages since education is endogenous with respect to the causal effect β. The potential for the unobserved characteristics which determine schooling choice to also be correlated with wage, has for a long time been a concern to labour economists. If we are to draw valid conclusions regarding the economic return to education we must isolate the *causal* effect of education on wages. Clearly this is not straightforward because of this concern: we anticipate that factors affecting the education choice an individual makes also independently affect their earned wage, we expect $\mathrm{E}(u_i\ \epsilon_i) \neq 0$.

Earlier research concentrated on the issue of 'ability bias' which suggested that $\mathrm{E}(S_i\ \epsilon_i) > 0$ because the residual picks up ability which is positively correlated with both wages and schooling. This ability bias explanation suggested that OLS was unambiguously biased upwards. In contrast, in his influential paper of 1977, Griliches proposed that measurement error in the schooling variable would lead to an attenuation of the OLS coefficient on schooling, biasing it towards zero. Griliches concluded that 'ability bias' was in reality small and was overwhelmed by the bias introduced by measurement error, with the result that OLS under-estimated the actual return to education. Card (1994) reported that in the micro-survey data commonly used by labour economists, measurement error in the schooling variable accounts for approximately 10 percent of the variance in observed schooling. This would lead to a 10 percent attenuation bias in the OLS coefficient – and even more if other covariates in the regression are correlated with the real level of schooling (Card, 1994). This estimate of around 10% concurs with evidence from Ashenfelter and Kreuger (1995) (using data from twins and reporting the measurement error to be between 8% and 12%), and furthermore, studies in which the education variable is deemed to be much more reliably measured (for example Uusitalo, 1999, in which the schooling information comes directly from school records) still find the IV estimates to be considerably higher than the OLS.

At the start of the 1990s, a number of economists suggested that OLS estimates of the return to education may suffer from a further bias – 'discount rate bias' (see Lang, 1993; Card, 1994).

In Becker's model of human capital formation, with standard assumptions[2], an individual will accumulate human capital to the point where the marginal rate of return on the last unit of education is equal to his/her discount rate. To illustrate this: (see Kling 2000) assume that the individual's earnings opportunities are summarized by the function $y = g(S)$ which specifies the earnings available for each level of education, S. Further assume that individuals earn nothing whilst in school, and discount the future at a constant rate r. Then in deciding upon the level of education to acquire, individuals will maximise the present discounted value of future earnings:

$$\int_S^\infty g(S)e^{-rt}dt = \frac{g(S)e^{-rS}}{r} \tag{3.3}$$

As standard in the literature, taking the log of this to be the individual's utility function over (S), having substituted y out of the utility function, gives:

$$U^*(S) = log(g(S)) - rS - log(r) \equiv log(g(S)) - \phi(S) \tag{3.4}$$

where $\phi(S) = log(r) + rS$. The optimal level of schooling is determined where the marginal benefit of an additional year of schooling is equal to the marginal cost, which is explicit in the first order condition:

$$\frac{g'(S)}{g(S)} = \phi'(S) \Rightarrow \frac{g'(S)}{g(S)} = r \tag{3.5}$$

If we further assume that $g(S)$ is log-concave then this solution equates the marginal rate of return to schooling with the individual's discount rate.

An individual's discount rate reflects both his/her access to finance to fund current investment in education whilst deferring earnings and also his/her rate of time preference. If individuals differ in their preferences and in their financial resources, this will result in different discount rates and lead to variation in the point at which they stop acquiring education – a higher discount rate resulting in a lower optimal level of education. Therefore schooling level choice may differ amongst individuals of the *same* ability because of differences in individual discount rates (Lang, 1993). The natural question that arises is: what effect will discount rate variation have on the OLS estimates of the return to schooling – does the unobserved discount

[2] i) workers maximize the discounted present value of lifetime wealth; ii) time in school is independent of time in work, or alternatively lifetimes are infinite; iii) there are no direct costs of education; iv) effect of experience on earnings is multiplicative.

rate that affects education also affect wages?

Intuition tells us that there is reason to believe that it might. It could be the case that individuals who have a higher discount rate because of their rate of time preference, have more ambition or determination to get into the labour market and earn money. This drive is rewarded in higher wages and also these individuals are more likely to choose career paths with steep wage curves. Consequently a higher discount rate is associated with lower education but also a higher wage controlling for education, thus $E(u_i\ \epsilon_i) < 0$. In this case the OLS estimation of the return to education is negatively biased. However, it may be that the opposite is true: Munasinghe and Sicherman (2000) present strong evidence from the NLSY[3] that smoking can proxy for rate of time preference, and that after controlling for a rich set of covariates, smokers (high discount rate individuals) experience lower initial wages and lower wage growth than non-smokers (low discount rate), which would suggest that high discount rate individuals are not selecting into steep wage growth occupations. If the wages of high discount rate individuals are lower (conditional on education) and grow more slowly then OLS estimates will be upward biased.

Discount rate and ability are both sources of variation in levels of schooling, moreover these two sources of variation interact in a complex way. Momentarily ignoring the demographic and background characteristics in X that affect schooling, the demand for schooling function is $S=S(a,r)$: schooling level choice depends positively on the individual's innate ability (a) and negatively on their discount rate (r). We can invert this function to get innate ability as a function of schooling and the discount rate: $a=a(S,r)$. So "...even if the discount rate and innate ability are uncorrelated, they are correlated once we condition on the level of schooling. For a given level of schooling, individuals with higher discount rates will have more innate ability" (Lang, 1993, p10). While a higher discount rate reduces an individual's level of schooling, when we hold that level of schooling constant, those with higher discount rates will have higher ability and this will be rewarded with a higher wage. Recalling the model, this makes sense: we know that if two individuals have chosen the same level of schooling it means that for each, at that point, the marginal return to schooling is equal to their discount rate. Thus the individual with the higher discount rate has a higher marginal return at that level of education, indicating that

[3] National Longitudinal Survey of Youth, US data

they have higher ability. Therefore a higher value of discount rate will reduce schooling, but conditional on schooling in the wage equation, a higher discount rate will mean higher ability and a higher wage: thus $E(u_i\ \epsilon_i) < 0$. Therefore this potential mechanism through which discount rate affects the joint process of education and earnings again suggests a negative bias in the OLS estimates. If both ability bias and discount rate bias affect the OLS estimate of the return to education but work in opposite directions, then *a priori* we cannot determine what the net bias in the coefficients will be. It is possible that the OLS is higher or lower than the 'true' return to education.

3.3 Solving the Endogeneity Problem

Over many years, economists have attempted solve the problem of the endogeneity of education in a number of ways. Firstly, a number of studies attempt to control for the effect of ability bias directly by including measures of ability such as IQ and other test scores in the model. However, aside from concerns over whether these types of variables are a good proxy for wage earning ability, Lang (1993) demonstrates that depending on the functional form chosen for the earnings equation, adding 'ability' variables to the model may not necessarily improve their explanatory power and in fact may result in perverse signs for the these variables. The variety of findings in the empirical literature (see Lang, 1993) for the signs and significance of these variables justified Lang's concerns. Moreover this 'ability' variable 'solution' does nothing to counter the problem of discount rate bias.

Another approach is to use twins or siblings and exploit differences in their education levels and earnings under the assumption that using twins (especially identical twins) or siblings, eliminates differences in innate ability, and provides an unbiased estimator of the return to education. However, Bound and Solon (1998) argue forcefully that the twins methodology is problematic, highlighting a number of non-trivial issues. Moreover, this strategy also constrains us to the assumption that twins/siblings are identical with respect to discount rates – which unlike ability (which is arguably genetic) is a taste parameter and so this would appear to be an even stronger assumption. A further concern for this approach is that when identification relies on differences in education, there are two points at which measurement error can occur, consequently identifying the return to education through differences in education is likely to be subject to greater measurement error (Harmon and Walker, 1995). Therefore it is far from certain that twin studies can offer a solution and return an unbiased estimate of the return to education.

An alternative strategy which has been the focus of much of the literature, is to identify a variable (or ideally a set of variables) which affect schooling but do not independently enter into the earnings equation and are uncorrelated with the error term in the wage equation. If such variable(s) can be found, then they can be used to construct instrumental variables estimates of the return to education. We will only arrive at a consistent estimator for the return to education if the model is statistically identified. Recalling the model from the start of this section, the moment conditions that we want to impose:

$$\mathrm{E}(X_i\epsilon_i) = 0 \Rightarrow \mathrm{E}((\ln w_i - X_i'\varphi - \beta S_i)X_i) = 0 \tag{3.6}$$

$$\mathrm{E}(S_i\epsilon_i) = 0 \Rightarrow \mathrm{E}((\ln w_i - X_i'\varphi - \beta S_i)S_i) = 0 \tag{3.7}$$

would be sufficient to identify the model's parameters – providing us with a consistent estimator for β. The corresponding sample moments provide K equations to estimate K parameters, therefore we can estimate $\hat{\varphi}$ and $\hat{\beta}$. However, when we know $\mathrm{E}(S_i\ \epsilon_i) \neq 0$ the lower equation no longer holds and we do not have enough equations to solve for the number of parameters to be estimated. The instrumental variables solution is to use the instrument to derive an additional moment condition that does hold, and replace $\mathrm{E}(S_i\ \epsilon_i) = 0$ and its corresponding sample moment condition with the new condition.

If such an instrument, $\hat{S}_i$, can be found then the violated moment condition can be replaced with $\mathrm{E}((\ln\ w_i$ - $X_i'\varphi$ - $\beta\ S_i)\hat{S}_i) = 0$. Provided $\hat{S}_i$ is not a linear combination of the $X_i s$ then the corresponding sample moment condition along with the other non-violated moment conditions will be sufficient to identify the parameters $\hat{\varphi}$ and $\hat{\beta}$.

Allowing heterogeneity across individuals in marginal costs of education (due to differences in discount rates) and in marginal returns to education, according to individual characteristics in the vectors X and Z, we can write:

$$\frac{g'(S)}{g(S)} = b_i; b_i = X_i\gamma_1 + u_{1i} \tag{3.8}$$

$$\phi'(S) = r = r_i + kS; k \geq 0; r_i = X_i\gamma_2 + Z_i\pi + u_{2i} \tag{3.9}$$

Marginal returns to school are constant, whereas the marginal costs of schooling are increasing in the level of schooling. This is plausible if individuals can finance education initially from family resources, then perhaps from government funding and later only through their own private sources, and if the time and psychic costs of education increase with the level of the qualification/education in question. Equating equations (3.8) and (3.9) gives an explicit solution for the optimal level of schooling:

$$S_i^* = \frac{b_i - r_i}{k} = X_i\tilde{\gamma} + Z_i\tilde{\pi} + u_i \tag{3.10}$$

We can get back to the Mincerian specification of the human capital earnings function (equation (3.1)) by integrating the marginal benefits of education over the years of education (and here we specify explicitly the heterogeneity in returns across individuals by allowing the β to vary with i):

$$\int_0^{s_i} \frac{g_i'(s)}{g_i(s)} ds = log(y_i) = a_i + b_i s_i = X_i\varphi + S_i\beta_i + \epsilon_i \tag{3.11}$$

In this model, we can have ability influencing individual earnings both through the individual intercept term a_i (this is the 'unobserved ability' that has been the focus of much of the literature), and through the marginal benefit of an additional year of education captured in b_i, which varies according to the individual's characteristics. Any candidate instrument must be independent of the individual ability intercept term a_i, which means that Z_i must be orthogonal to ϵ_i (and indeed to u_i). The IV estimate – based on 2SLS in which the first stage is estimated by (3.10) and the second stage is estimated by (3.11) – of the schooling coefficient β is a weighted average of the marginal returns to education (the β_i) for those whose schooling choice is influenced by the instrument, conditional on X. In order to give this LATE interpretation, there is a monotonicity requirement that all individuals have the same signed response to the instrument i.e. in the case of RoSLA this is that $\tilde{\pi}$ is greater than or equal to zero for all individuals i.e. no-one chooses less education as a result of the change in the minimum school leaving age.

There is a large literature in this area in which a number of instruments have been used. Many studies are reviewed in Card (2000). Some studies exploit institutional features or policy changes while others rely on variations in costs across individuals (in each case these instruments alter the marginal cost functions r_i). The latter includes instrumenting using college proximity (for example, Card, 1995), while the former group includes the seminal Angrist and Krueger (1991) paper exploiting differences in schooling owing to the interaction of quarter-of-birth and state variation in when children have to commence compulsory schooling.

While IV has the advantage that we can potentially derive estimates purged of the biases discussed above, it also has some shortcomings. Weak instruments (that is, those that although uncorrelated with wages are hardly correlated with schooling) and invalid instruments (those that although correlated with schooling, may also be correlated with wages) may be worse than no instruments at all.

Some authors (Staiger and Stock, 1997, and Bound *et al.*, 1995) have highlighted that many existing instrumental variables studies have been undermined by a lack of precision in their first stage estimates. If the instrument used is only weakly correlated with the endogenous regressor (schooling) then the IV estimates are potentially as biased as the OLS estimates. Bound and Jaeger (1996) show how quarter-of-birth interactions with state and year, used in Angrist and Krueger (1991), form weak instruments that cause IV to be *more* biased that OLS.

Further, Bound *et al.* (1995) show that even a small correlation between the instrument and the error term in the wage equation can result in a large bias in the IV estimates even in large samples. This problem is compounded if the instrument is weak: the magnitude of the bias in the IV approaches the bias in the OLS as the R^2 from the first stage regression of the endogenous explanatory variable on the instruments approaches zero. While this R^2 statistic in not routinely reported, the problem of weak instruments may be quite prevalent since most of the IV studies surveyed in Card (2000) suffer from imprecision and the IV returns are not significantly different to those from OLS. Since the work of Staiger and Stock, Bound *et al.* and more recently Stock and Yogo (2004), it has become more common to report the first stage R^2 and the F-statistic on the exclusion of the instruments from the first stage, which help to establish the relevance of a candidate instrument. Therefore it it is crucial to establish that there is a strong relationship between the instrument and the endogenous regressor (schooling) i.e. that the instrument is relevant.

However, it is not routinely possible to test an instrument for correlation with the error term in the wage equation (i.e. test the validity) as to do that we would first need to estimate the wage equation to give us a valid error term which requires a consistent estimator for φ and β, but we can only find a consistent estimator if we have an alternative instrument that we know is valid and strong in the first place. The advantage in having multiple instruments – as I have in this study – is that this allows me to determine the validity of the preferred instrument (early smoking), exploiting the validity of the other instrument available (RoSLA).

An additional problem with the IV strategies is that what they capture is a 'local average treatment effect' (LATE), as outlined above in the formal modelling. The basic problem is that while OLS provides an estimate of the average marginal return to another year of schooling, the IV estimator provides a weighted average marginal return to another year of schooling with the weighting determined by the extent to which individuals' behaviour is changed by the 'treatment' (Angrist and Imbens, 1995). Card (1998) notes that depending on whether the marginal returns to education for individuals in the 'treatment' group are higher or lower than the average marginal return to education, the IV estimator may over- or under-estimate the average marginal return to education for the population as a whole. In these circumstances it is not possible to generalise from the IV estimates to all individuals. Prior to Angrist and Imbens formalisation of LATE reasoning, Lang's (1993) paper, in which the term 'discount rate bias' was first used, criticised Angrist and Krueger (1991) on the basis that what they were identifying was in fact a LATE, though Lang termed it 'discount rate bias'. Kling (2000) has demonstrated how Card's 1995 paper using the proximity of a four-year college to instrument for education does indeed capture the return for less advantaged families whose schooling decisions were most effected by the reduced cost associated with a college being nearby. This was Card's intuition in the paper, and Kling has formally shown that Card's estimates do indeed capture a LATE. This is not necessarily a problem – the estimate is not invalid, however it does affect the interpretation. Card captures a LATE and from a policy perspective it is an important LATE to know.

I have already argued that, for a given level of education, those with higher discount rates will have higher ability. Therefore when we take a given level of education, for example the 10 years education that was the minimum prior to the date when the school leaving age in England was raised from 15 to 16, those with high discount rates will have greater ability than those who choose to leave at 15 because of low returns to education. Thus to the extent that individuals in the low education group have high discount rates because of higher than average costs of education rather than lower than average returns to education, LATE reasoning suggests that IV estimates that isolate this group will find returns that are higher than the average marginal return to education, and may be higher than the OLS estimates (Lang, 1993; Card, 2001).

Alternatively, one could argue that the majority of individuals in this group whose behaviour is affected by the raising of the school leaving age, are low discount rate, low ability and would have located at the minimum prior to the raising of the school leaving age because their return to schooling has fallen to the same (low) level as their discount rate already. In this case, we would expect that the IV estimates of the return to education would be below the average marginal return to an additional year of education. Figure 3.1 shows the education leaving age density when the minimum school leaving age is 15 compared with when it is 16. It is clear that in the upper ranges the densities are very similar, and that the increase in minimum school leaving age affects only the lower part of the distribution of leaving ages. This concurs with the evidence of Chevalier *et al.* (2004) who use a large sample of data from the General Household Survey (GHS) and find – using a number of tests of the equality of distributions – that RoSLA only affected the attainment of those at the bottom of the schooling distribution, there was not a ripple effect further up. Similarly, Oreopoulos (2006) concludes that the earlier (1947) RoSLA only affected the lower part of the distribution, and Harmon and Walker (1995) using both the 1947 and 1973 RoSLA find that only the lower portion of the distribution is affected. Whether these individuals affected by the policy are predominantly high discount rate or predominantly low ability will determine whether we expect the IV estimate from the raising of the school leaving age to be higher or lower than OLS.

Therefore it is important to identify an instrument that avoids these three prominent problems: being correlated with the structural equation error term, being only weakly correlated with the endogenous regressor or capturing a LATE that is not informative when it comes to answering the question we want to ask – what Murray (2006) terms the bad, the weak and the ugly instruments.

3.4 Instrumenting Education Using Early Smoking

3.4.1 Theory

Evans and Montgomery (1994) proposed using whether or not an individual smoked when they were young as an instrument for schooling[4]. The intuition for the instrument starts from the acknowledgement that just as schooling is not randomly assigned across the population, the decision to engage in (un)healthy habits is not randomly distributed. Evans and Montgomery note that "one of the most persistent relationships in health economics is that more educated people have better health and better health habits" (1994, p1). This view is supported by a number of reviews of the empirical evidence on the link between health and education by Grossman (see Grossman, 2005). After extensively reviewing the evidence Grossman concludes that that completed years of formal schooling is the most important **correlate** of good health, and this statement applies whether health is being measured by mortality rates, morbidity rates, self-evaluated health status or psychological well being (Grossman, 2000). In the UK, Oreopoulos (2006) uses data from the General Household Survey (GHS) which asks individuals to self-report their health status, and finds that an additional year of schooling increases the chance an individual will report good health by 6.0 percentage points, and reduces the chance of reporting poor health by 3.2 percentage points. There remains a debate as to whether or not this education-health relationship is causal i.e. through more education people learn the dangers of poor health habits and are thus less likely to engage in them, with Evans and Montgomery citing a quite different explanation for the relationship due to Victor Fuchs (1982). Fuchs argues that unobserved differences in the rate of time preference determine both the number of years schooling that an individual attains and their investments in health, as both decisions involve a trade off between current costs and the discounted value of future benefits.[5]

As with Becker's model of human capital accumulation, in a health accumulation model individuals invest in health until the marginal return to health investment equals their discount rate. If an individual has a higher discount rate because of her rate of time preference, she cares

[4]This IV strategy has also been pursued by Chevalier and Walker (2001) using GHS and National Child Development Study (NCDS) data, and by Fersterer and Winter-Ebmer (2002) for Austrian data.

[5]It is worth noting that the explanations of the health/education correlation as being causal or driven by unobserved time preference are not mutually exclusive: it may be that education promotes better health habits or improves the efficiency of health inputs but individuals may still choose to act differently in light of this education according to their rate of time preference.

less about the future and more about the present and will therefore *ceteris paribus* quit formal education at a younger age and be less likely to invest in good health habits (and be more likely to engage in unhealthy habits). If the correlation between health habits, such as smoking, and education is driven by a common unobserved factor (time-preference) then some health habits could potentially be used as in instrument for education.

Not all health habits can be used as an instrument for two reasons. Firstly, some health habits have consumption as well as investment value. Going to the gym or playing squash for example, have consumption value and are likely to be correlated with family income/background and possibly correlated with the unobserved component of earnings. Secondly, some health habits such as heavy drinking or drug abuse would be unsuitable as they are likely to have an effect on current wage through their effect on productivity. I follow Evans and Montgomery in arguing that smoking as a teenager is a health habit that can be used as a valid instrument for education.

The decision that an individual makes at age 16 as to whether to continue in education or not is likely to be significantly affected by his/her discount rate – whether that is because of access to financial resources or because of the individual's rate of time preference. In the UK this is the first point at which individuals can choose to leave education, moreover it remains the case that staying in school post-16 and taking A-levels is still the major route into university, therefore the decision to remain at school at 16 is likely to be affected by the individual's discount rate. Moreover, whether an individual chooses to smoke at 16 is also likely to be determined in large part by their rate of time preference. Whether I look at the largest sample of working age men available in the BHPS or my estimation sample it is the case that of the individuals who have ever smoked, approximately 61% were smoking when age 16, and approximately 80% were smoking when age 18[6]. Therefore it is clear that the majority of individuals who ever smoke, first take that decision at around the same time that they are making decisions over the continuation of their education. Evans and Montgomery find that the concurrence in the timing of the smoking and school leaving decisions generates a statistically precise and quantitatively large correlation between years of education and early smoking and, unsurprisingly, the same relation is found in UK data. Thus smoking at 16 satisfies the first criterion for an instrument: it

[6]The precise figures for the estimation sample (largest possible sample) are 60.47% (61.00%) smoking at age 16, 81.11% (79.73%) smoking at age 18.

is relevant as it is strongly correlated with completed education. Moreover, as will be illustrated below, the effect of early smoking on years of schooling is sizeable (just under one year less education is completed on average by those who smoke when 16 *ceteris paribus*), therefore the instrument works through a substantial variation in education (Angrist and Krueger, 1991, in particular has been criticized on the basis not only that the correlation between their instrument and education is low – i.e. low t-statistic(s) on the instrument(s) – but also that it induces only a very small variation in education attained, approximately only 0.1 years of education).

The second criterion is validity: the instrument must not be correlated with wage. As I am using a past health habit, smoking at age 16, to instrument for education in the equation for *current* wage, there should not be a correlation via an income effect: the contemporary wage can have no impact on the disposable income of 16 year old deciding whether or not to smoke. Moreover, theoretically whether one smoked at 16 should have no independent direct effect on *current* wage. It is by no means certain that current smoking affects current wage via a productivity effect, thus a link between smoking at 16 and current wage would be even more speculative. So there is no reason to think that smoking at 16 would affect current wage – and as individuals age and move further away from being 16 this is even more so the case. Moreover, there is a good degree of movement between smoking and non-smoking amongst my sample of men, with 42.0% of men who *did* smoke when they were 16 having stopped by the time they are first observed in the data, and 38.4% of the men who are smokers when first observed in the data *were not* smokers at age 16. Given these arguments I believe that smoking at 16 can legitimately be excluded from the wage equation.

However, due to the very nature of the unobservables in the wage equation, it is not possible *a priori* to rule out a correlation between smoking at 16 and the unobservables that do affect wage. If the rate of time preference that characterises early smokers does lead them into higher than average wage jobs (as one part of the discount rate bias story suggests) then this would invalidate the instrument and the estimates derived would continue to be biased. Whether or not the instrument is valid is an empirical point, and usually it is not possible to formally test for the validity of an instrument. Fortunately, given I have more than one instrument I have an over identified system and can therefore test the validity of the instruments. In section 3.10 I test the validity of both instruments and cannot reject the null hypothesis that the instruments are indeed valid. Moreover, I can use the RoSLA instrument to just identify the system and also include early smoking as an explanatory variable and find that it does not have a significant coefficient in the wage equation, which again indicates that it can be excluded from the structural equation. Both of these tests are predicated on the assumption that the RoSLA instrument is valid, which I do not believe is a strong assumption given that the raising of the school leaving age was an exogenous policy change. If we accept that early smoking satisfies these two criteria of relevance and validity then an indicator for early smoking can be used as an instrument: it can be the Z_i in equation (3.10), influencing schooling through changing the marginal costs of schooling in a way which is orthogonal to ability.

3.4.2 Is it a spurious relationship?

This observed relationship between smoking at age 16 and educational attainment could be driven by something other than rate of time preference, something that also affects wages and therefore makes the instrument invalid. It could be argued for example, that poorer socio-economic background lowers education and increases the likelihood of smoking – i.e. smoking at 16 is more a reflection of socio-economic background than discount rate. Clearly socio-economic background may influence the decision to smoke at 16, however, my preferred specification of the model includes variables to control for background characteristics at the time that the individual was a teenager and therefore should take this effect out of the coefficient on the early smoking indicator. If it is the case that smoking at 16 is channelling the effects of such characteristics then adding background characteristics into the schooling demand equation would seriously reduce the impact and significance of the smoker at 16 variable. As it is, the coefficient on smoker at 16 changes only from -1.08 (with a standard error of 0.11) to -0.88 (s.e. 0.11) when we add in the background characteristics. The background characteristics that I am able to include are dummies for the occupational class of each parent when the individual was 14, and a dummy to indicate whether the person lived with both natural parents from birth up until the age of 16. These variables should do a good job of capturing the individual's background socio-economic circumstances at the time when they are making decisions over education (and whether or not to smoke). Therefore the fact that when they are included in the model, the smoker at 16 indicator still has a quantitatively large effect on schooling and is precisely estimated suggests that it is not socio-economic background that is picked up in the early smoker indicator.

Like Fuchs, in their work on rational addition Becker and Murphy (1988) posit that the decision to smoke reflects discount rate in that it indicates the rate of time preference and this is what I argue – that smoking at 16 reflects rate of time preference. One way in which Fuchs supported his hypothesis was to show that education at age 24 when education levels vary considerably, is as important a predictor of smoking at 17 – when most individuals have the same level of education – as it is a predictor of smoking at 24 (see Farrell and Fuchs, 1982). Using a larger dataset than my actual estimation sample, I implement a probit of current smoking using completed years of schooling amongst the explanatory variables, and repeat the probit for smoking at age 16. The marginal effects estimated at the mean of the explanatory variables suggest that for each additional year of schooling the probability of being a current smoker falls by 2.7% (significant at below the 1% level). In the probit for smoking at 16, it is estimated that each additional year of completed education reduces the probability of having smoked at age 16 by 3.8% (significant at the 1% level) (see Table 3.1). Thus completed education is a significant determinant of early smoking – suggesting that it is not greater education that determines the decision (not) to smoke – education predicts early smoking as well as later smoking, suggesting that another underlying factor (time preference) is determining both.

Moreover, with regard to the question of whether it is a knowledge effect, it is less likely to be the case that the education-smoking link is causal, to the extent that formal schooling is not the main avenue through with knowledge of the detrimental (indeed potentially fatal) health consequences of smoking are disseminated. Since the mid-1960s, the negative effects of smoking on health have been known and increasingly communicated to the public via various awareness campaigns and indeed successive governments have been increasingly direct in their discouragement to smoke both via taxation and the media. As a result, it is decreasingly likely to be the case that only through continued education (past the compulsory level) that individuals are made aware of the negative health effects of smoking. The hypothesis that the relation between education and smoking is in fact driven by the time preference of the individual rather than being a causal or knowledge effect can be tested and this is something that I return to in section 3.8.

The correlation between smoking and education is also consistent with an alternative hypothesis: that those with lower unobserved ability will acquire less education and are more likely to smoke. I have outlined how ability and discount rate bias interact in a complex fashion thus it is difficult to completely disentangle the different effects. There is no doubt that to the extent that smoking at 16 is correlated with lower ability there will be some effect of ability picked up by the smoking at 16 variable. However, if it is the case that we are primarily picking up some measure of ability then we would expect that – by definition – smoking at 16 only affects the education of individuals at the lower end of the ability distribution. If we assume that the residual from the OLS log wage regression is a reasonable proxy for ability, we can divide this residual wage distribution into quintiles and examine whether smoking at 16 is a feature only of low ability (low residual wage) individuals or if it is something that individuals of all abilities engage in.

Table 3.2 shows the numbers who smoke at age 16 in each quintile of this residual log wage distribution. The left-side panel of the table shows that in the lowest quintile approximately 44% of the males smoked at 16. This figure falls to approximately 39% in the next quintile up and the next after that (30%) before rising again in the fourth quintile (34%). Despite a fall in the last quintile, the figure for the percentage of individuals who smoked at age 16 are still as high as 23% in the highest quintile of the residual log wage distribution. There are fewer smokers at 16 in the higher quintiles of the distribution but that is to be expected, given that smoking at 16 is likely to be in some part be correlated with lower ability. Nevertheless there remain substantial numbers of smokers at 16 in the highest quintiles of the residual log wage distribution which indicate the highest ability individuals.

In addition, Figure 3.2 plots the density of education leaving age for smokers at 16 and non-smokers at 16. If it was only low educated, low ability individuals who smoke at 16 then we would expect the densities to look very different with very little mass in the upper ranges for the early smokers. However, while the non-smokers at 16 density does have a greater mass around 21 and less around 15/16 suggesting more non-smokers go to university, there is still a non-negligible proportion of smokers at 16 in the right tail of the distribution. This is consistent with the idea that A-levels are the main route into university – we would expect that more lower discount rate individuals to remain in school at 16 and the result of this is the lower percentage leaving at 16 and the resulting higher percentage leaving at around 21. Elsewhere the picture is similar but with the smokers at 16 distribution to the left of the non-smokers. This is consistent with the discount rate hypothesis which says that there are smokers and non-smokers at 16 of all abilities and that smoking at 16 has an effect to reduce education at all points of the ability distribution.

It is certainly true that younger cohorts have consistently acquired more education, and for the men in my sample, smoking at 16 has generally been decreasing: 39.8% of the cohort born in the 1940s smoked when 16, this fell in successive cohorts to 30.0% (those born in the 1950s), 27.8% (60s) before rising again amongst those born in the 1970s, of whom 36.3% smoked when 16. This general pattern would also lead to a shift of the curve to the right for non-smokers at 16, therefore to be sure that it is the case that smokers at 16 do get less education than non-smokers at 16, Figure 3.3 produces the same plot for the cohorts born in the 40s, 50s, 60s and 70s (which accounts for 88.0% of the men in my sample)[7]. For each cohort the picture broadly follows the pattern of Figure 3.2: there is more mass in the right tail for non-smokers but still a sizable density in the right tail for smokers at 16. This illustrates that for all cohorts there are smokers at age 16 across the entire distribution of education levels, but that smokers at 16 acquire less education on average[8].

Therefore in answer to the criteria for a suitable instrument: early smoking is not "bad", there is no reason to suspect that smoker status at 16 should violate the exclusion restriction (and this is something that I test, see section 3.10, to ensure the instrument is valid); it is not "weak" as there is a strong, very significant and sizeable *ceteris paribus* effect of early smoking on years of schooling; and it is not "ugly", though it captures a LATE – the group of individuals who have lower education because of a higher than average discount rate – this is a group comprised of individuals of all abilities and is therefore an informative group to consider the return to education for, arguably more representative of the population as a whole than groups identified by other IV estimation strategies.

[7]The corresponding graph for individuals born in the 1930s reflects a similar pattern but only accounts for 9.4% of the sample

[8]The cohort born in the 1970s have a restricted education leaving age in that the majority of this cohort are 22 years old or younger, hence their distribution is slightly truncated.

3.5 Data

I use the British Household Panel Survey (BHPS) which is a nationally representative survey of the population which began in 1991 and follows the sample individuals each year. In 1999 in addition to the core survey there was a supplementary component in which questions were asked regarding previous health habits. I have constructed an 15-wave pooled-panel dataset containing variables describing individuals' characteristics, a dummy to indicate whether the individual smoked when 16, education, and current hourly wage rate. Since the previous health habits question was only asked in wave 9, I only have observations from individuals present in wave 9, but I have all waves of observations for these individuals. I include males who are in full-time employment (30+hours per week), are not self-employed and are in the age range 19 to 65 inclusive[9].

There are issues of measurement error when using number of years of schooling as the measure of education, however in order to make my results comparable with the majority in the literature I use the observed number of years of schooling as my education variable[10]. The BHPS does not ask how many years education an individual has nor when the individual first left full-time education, rather it asks the age at which the individual left school and age at which he/she left further education. As I construct my years of schooling variable from age when left school or age when left further education if the individual went on to further education, I encounter problems when people return to full-time education after a number of years away. If an individual completes GCSEs, A-levels, a standard 3-year degree, then a Masters degree and then a PhD (3 years) this would equate to 21 years of education, therefore I exclude any individual with more than 21 years recorded education. This excludes observations from just 84 individuals (3.6% of those with years of schooling calculated).

With respect to earnings, it is standard to use the log of hourly earnings and so again for comparability this is what I have constructed – the log of real wage (using 2006 pounds as the base)[11]. I trim the log wage distribution such that the top and bottom 1% within each year are

[9]This age range captures 'prime-age' males and ensures that smoking at 18 is not the same as current smoking for any individuals, as smoking at 18 will be used as an instrument as evidence in support of the rationale behind the early smoking instrument.

[10]Formally: Years-of-schooling = (age left education - 5); thus I assume a school start age of 5, which is the compulsory school start age in the UK.

[11]Current hourly wage is not explicitly recorded, however following other BHPS users (for example Booth and Frank (1999)) I constructed the natural log of hourly wage rate by constructing hourly wage as: $w_i = \text{PAYGU}_i$

excluded.

The dataset constructed contains 21,256 observations from 2,266 males with each individual having between 1 and 15 observations; the mean number of observations per individual is 9.38, median 10[12]. Table 3.3 contains summary statistics for the estimation sample, with the breakdown by early smoking status in Table 3.4.

3.6 Estimation

I cannot exploit the panel to eliminate unobserved ability since completed years of education is a fixed effect but I can use the repeated observations to improve precision – although I need then to adjust the standard errors to take account of there being repeated observations of the same individuals at different times[13]. I do this by allowing clustering for each individual in the variance-covariance matrix which allows for there to be a correlation between the error terms for each individual but no correlation between the error terms of different individuals. The robust standard errors generated do not impose any assumptions on the functional form of the potential correlations and heteroskedasticity controlled for in the error.

I aim to produce estimates that are comparable with other research so I begin by estimating a conventional human capital earnings function where the dependent variable is the natural log of real hourly wage, and the explanatory variables are age, age-squared, and years-of-schooling. I also include controls for ethnicity, for region (using the 13 standard regions) in order to pick up regional effects such as real wage differentials, year-of-birth[14] and its square to pick up cohort effects[15] and dummies for parental characteristics. As discussed, I include parental characteristics because in their absence, the smoking at 16 variable could be picking up background

$/\{4.33(\mathrm{JBHRS}_i + 1.5\mathrm{JBOT}_i)\}$ where PAYGU_i is gross monthly earnings before tax and other deductions in current main job; JBHRS_i is standard weekly hours worked; and JBOT_i is overtime hours worked each week. It is assumed overtime is paid at 1.5 times the normal hourly wage, $4.33 \approx$ no. weeks per month. Therefore $w_i =$ (Monthly Gross Earnings/No. hours worked per month) = Hourly wage rate.

[12] I order to avoid issues around differential attrition, I have re-estimated the models using both inverse probability weighting and also including in the regressions a variable indicating the number of observations that each individual has, and in each case the results remain, available from the author.

[13] As the first stage involves regression of years-of-schooling – which is time-invariant– on characteristics, I re-estimate the model using just one observation (their first) for each member of the sample but then all of the observations in the second stage, bootstrapping to get the correct standard errors in each stage. The results for the early smoker instrument and for the RoSLA instrument are in the appendix Tables B.2 and B.3 respectively. There is no substantive change in the conclusions.

[14] Year-of-birth is rescaled such that 1897=1,. . . , 1989=93, since in the range 1897-1989 the birth years in my total dataset, year-of-birth and year-of-birth-squared are perfectly collinear.

[15] Including a higher order polynomial in a suitably rescaled year-of-birth does not alter the results nor add to precision in the estimates and so in the interests of parsimony only a quadratic is used.

characteristics correlated with education and smoking at 16. As mentioned above, the parental characteristics variables that I have are the standard occupational classification of the job of both the individual's father and mother when the individual is 14 years of age, and a dummy to indicate that the individual lived with both natural parents from birth up to the age of 16. Including year dummies in the model would be problematic since I include both age and year-of-birth, however I do include controls for whether it was the early-, mid-, late-1990s or post-2000 to allow for business cycle effects [16]. Mincer's specification of the human capital earnings function, included experience and experience-squared. In the absence of information on labour market experience, Mincer suggested potential experience – age minus schooling minus 6 (assuming individuals begin schooling aged 6) – could be used as an approximation. However, using this approximation would mean that measurement error in the education variable would necessarily transmit into the experience and experience-squared variables and moreover, the endogeneity of schooling (our main concern) will lead to potential experience and its square being endogenous, resulting in three endogenous regressors. Age and age-squared are the standard candidates to use as instruments for experience and its square, and are widely used as such, therefore this is the approach that I have taken.

I estimate the model first by OLS. I then implement the IV regression using the smoker at 16 indicator as the instrument generating the variation in years-of-schooling.

3.7 Results

The first column of Table 3.5 reports the OLS estimate of the human capital earnings function, the second column reports the IV results using smoking at 16 as the instrument. The third column reports the results from the reduced form equation for years of schooling. Looking at the third column of Table 3.5 we can see that individuals who smoke when they are 16 have on average 0.88 fewer years of schooling than those who do not smoke when they are 16. The robust standard error is 0.108 giving an absolute value of the t-statistic of 8.13. Therefore smoking when 16 is strongly significant for education, and the parameter precisely estimated. This is encouraging given the concerns raised by Staiger and Stock (1997) and Bound *et al.* (1995)

[16] These dummies are significant in the wage equation, though their inclusion/exclusion does not alter the coefficient on the instrument (1st stage) or $\hat{S}_i$ in the second stage

concerning the precision of first stage estimates. The R^2 of 0.246 is higher than the R^2 for first stage regressions in some other IV studies[17], and the F-statistics of 66.17 suggests a very strong instrument. The partial-R^2 of the effect of the instrument on years-of-schooling having partialled out the effect of the other covariates is 0.0289 which is high relative to the guidelines given by Bound *et al.* (1995). The high F and R^2 statistics suggest that the bias inherent in IV estimation in finite samples will be smaller than the OLS bias.

Therefore controlling for parental characteristics and year-of-birth, smoking at 16 reduces education by almost 1 year and is precisely estimated. The coefficients on year-of-birth and year-of-birth-squared suggest that from the 1920s onwards, later year of birth is associated with a greater number of years of schooling until the mid-1950s at which point this levels off for a decade before starting to decrease. Turning to the parental occupation dummies, we can see some significant effects on years of schooling[18], particularly for the father's occupational class. As we might expect almost all of the higher occupational strata dummies (the lower numbers) are associated with sizeable positive effect on an individual's education and are precisely estimated. This is particularly true of management (1), professional occupations (2) and associate professional/technical occupations (3), increasing education by 1.1 and 2.3 and 1.5 years respectively. Much fewer of the mother's occupation variables are significant, though a mother in a professional occupation (2) has sizeable positive and significant effect on education (associated with 1.4 years higher education). The fact that these parental characteristics dummies are strongly significant in the schooling equation but then not significant in the IV wage equation suggests that parental characteristics have a strong influence on education controlling for discount rate, but then controlling for education these parental characteristics do not influence wage.

Turning to columns 1 and 2, the OLS estimate suggests that an additional year of schooling increases wage by 4.6% whereas the IV estimate suggests the return is 12.9%. We expect that the IV results will be less precisely estimated than the OLS, and while the robust standard error on years of schooling in the instrumented regression is higher at 0.020 compared to 0.003 in the OLS regression, this still gives a t-statistic of 6.31 and is therefore still precisely estimated and significant at all conventional levels. The dramatic difference in the estimated coefficients

[17] Harmon and Walker (1995) for example have a first stage R^2 of 0.147.

[18] The omitted category are plant or machine operatives.

suggests that years of schooling is an endogenous variable, and this conclusion is strengthen if I include the residual from the first stage reduced form equation as a regressor in the OLS estimates, providing a Hausman test of the endogeneity of schooling. The absolute value of the t-statistic on this residual is 4.78.

There is nothing unexpected in the coefficients on the other variables. The dummy for the South-East region is significant in both the OLS and IV wage regressions, and is precisely estimated in each. Since the South East region contains London, it is expected that there will be a positive coefficient on wages given the London weighting. The R^2 for the OLS regression of 0.265 is comparable to other IV studies[19]where it is usually in the range 0.25 to 0.35. Though the R^2 for the instrumented regression is lower at 0.072 the fact that I am using instrumental variables suggests that goodness of fit is not what I am primarily seeking, my main concern is to find a consistent estimator of the causal effect of education on earnings and that is what the instrumented regressions allow me to estimate.

Estimation of the IV using the Fuller LIML estimator rather than 2SLS or IV-GMM, in order to be as robust as possible to any bias in the IV estimates, does not result in any notable change to the estimated coefficients or standard errors (the return to schooling in the IV estimation becomes 12.8 rather than 12.9), and the Kleibergen-Paap Wald statistic suggests the there is minimal bias in the IV coefficients.

3.8 Analysis

The results that I find are in line with those found in other studies. Angrist and Krueger (1991) find a return to schooling of 7.0% by OLS rising to 10% by IV (quarter of birth and state interactions). Card (1995) finds an increase in the estimated return to schooling from 7.3% by OLS to 13.2% by IV (college proximity). In studies using UK data, Harmon and Walker have consistently found results similar to my findings: using Family Expenditure Survey (FES) data for 1978-1986 they find estimates of 6.1% by OLS and 15.3% by IV (RoSLA, 1995), using the NCDS[20] they find estimates of 5.0% by OLS and 9.9% by IV (peer effects and education system level effects, Harmon and Walker, 2000), and using the GHS data they find results of 4.9%

[19] Card (1995); Angrist and Krueger (1991); Harmon and Walker (1995).
[20] National Child Development Study.

by OLS rising to 14.0% by IV (RoSLA and educational reforms, Harmon and Walker, 1999). Chevalier and Walker (2001) find using an earlier smaller sample of BHPS men (using just 6 waves) an OLS estimate of 6.4% rising to 20.5% using IV (RoSLA). Chevalier and Walker also construct estimates using smoking status at 16 and NCDS data, estimating a return of 6.1% by OLS, rising to 8.0% by IV; and using GHS data they estimate an OLS return of 6.4% rising to 9.5% when instrumenting using smoking status at 14/16/18.

More recently Oreopoulos (2006) has used the 1947 raising of the school leaving age in Britain, from 14 to 15[21], and GHS data to compute a standard IV estimate and a regression discontinuity IV estimate of the return to schooling, arguing that as this was a reform which affected around half of the population, the estimated LATE is closer to the average treatment effect (ATE). Oreopoulos estimates the return for British males (aged 32-64) to an additional year of education to be 5.5% by OLS, rising to 9.4% by IV, though the IV estimate is imprecisely estimated. Implementing a regression discontinuity design, Oreopoulos estimates a return of 15.0% for men, though again the estimate is rather imprecise[22].

Therefore my results of 4.6% by OLS rising to 12.9% by IV are of similar magnitude to the studies above, particularly the Harmon and Walker (1995).

3.8.1 Testing for a spurious relationship

Is it a background effect?

As outlined in section 3.4.2, it may be argued that the early smoker indicator is picking up differences in background characteristics between those who do and do not smoke at 16, and that these characteristics also affect wage. Hence the need to control as much as possible for socio-economic characteristics of the individuals at the time that they are making their decisions over education and smoking. As a robustness check I also estimate the model without the parental characteristic variables included, see Table 3.6. As alluded to in 3.4.2, the effect of removing the background characteristic variables is that the coefficient on the smoker at 16 indicator in the reduced form schooling demand equation increases to -1.08 (robust s.e. 0.113,

[21] Harmon and Walker, 1995, exploit both this 1947 raising of the school leaving age, and the later increase from 15 to 16 in 1973 to derive their IV estimates.

[22] When Oreopoulos implements his IV and RD models for *all* individuals – i.e. including females as well as males – the estimated returns are precisely estimated (1% level) and suggest returns of 14.7% (RD) and 15.8% (IV).

t =-9.61). The F-statistics on the exclusion of the instrument is 92.39, with the partial-R^2 of the instrument of 0.0400, again both suggesting a strong instrument, with a total R^2 for the first stage of 0.143, again comparable with similar studies. The estimated return to education in the wage equation is 12.1% (robust s.e. 0.016, t =7.56). Therefore while the nature of the result remains unchanged, it is clear that the parental characteristics variables inclusion is not driving the result.

Is it a knowledge effect?

It could be argued that the correlation between health and education is indeed a causal relationship: individuals with more education have more knowledge of the health consequences of certain habits and are less likely to engage in them. However, as outlined above, to the extent that public information campaigns have made the health risks of a particular habit known to the majority of the population, the correlation between education and that habit is more likely to be due to variations in unobserved factors such as discount rates. In the 1940s and 1950s we would expect less of a correlation between smoking and education, because smoking was not known then to be an investment in health choice. However, given the vast amount of information available to the public since the 1960s concerning the risks of smoking, it is fair to deduce that compared to other health habits, the correlation between smoking and schooling is more likely to reflect individuals' investment choices driven by time preference rather than knowledge.

To test this hypothesis, we can postulate that if there has been an increase in the general availability to the public of information on the risks of smoking, then we would expect that decisions to smoke at age 16 taken after the effects were widely known are more likely to reflect differences in discount rates, thus the negative correlation between schooling and smoking at 16 should be higher for individuals who reach 16 after the effects of smoking were widely known. If however the link between smoking and education is due to knowledge effects, after the knowledge of the effect of smoking is widely known, the correlation should disappear. The first Surgeon General's report highlighting the health consequences of smoking was published in 1964, therefore I have repeated the estimation and rather than including smoking at 16 alone as an instrument, I interacted this variable with a dummy indicating that the individual turned 16 before the report was published and a dummy indicating that the individual turned 16 after the report was published (i.e. in 1965 or later). If the relationship becomes stronger i.e. if the t-statistic on the smoking term interacted with the turned 16 post-1964 indicator is greater in absolute value than the turned 16 pre-1964 interaction term this would suggest that the relationship is reflecting differences in discount rates.

In Table 3.7, I report the first stage regression coefficients on these interaction terms when we use these terms rather than just smoking at 16. We can see that both the interaction terms are precisely estimated, significant and that the term for individuals who turned 16 in 1965 or later has a coefficient which is larger in absolute value by 0.1 years of education and has a substantially lower standard error, thus suggesting a stronger relationship post-1964.

A further test of the hypothesis that there is a causal link between education and smoking is to remove individuals who have less than the 11 years of education that the majority of individuals should have by the time that they are 16 and make the decision over whether or not to smoke and whether to continue in education. Re-estimating on this sample produces the results in Appendix B Table B.1. As can be seen, estimation on this sample does not alter the coefficient on education.

Is it an ability effect?

Another issue is the question of whether smoking at 16 is just picking up differences in ability. As already discussed, if smoking at 16 was picking up (lack of) ability, we would not expect that smoking at 16 would occur across the whole residual wage distribution as we have seen that it does – non-trivial numbers smoked at 16 in the upper quintiles of the log wage residual distribution. If we continue to use the residual wage distribution as a proxy for ability and, again dividing it into five quintiles, look at the first stage reduced form schooling equations, we can see that the effect of smoking at 16 is actually increasing as we move up the distribution. The left side of Table 3.8 shows that in the lowest quintile, schooling is reduced by 0.77 years, this is equivalent to a reduction of 6.21% of the mean number of years of education in this group. In the second and third quintiles the reduction in education associated with early smoking is even greater both in absolute terms and relative to mean education in these quintiles. The fourth quintile is affected the least by early smoking but still it is associated with three-quarters of a year less education, and in the highest quintile the estimated reduction is 0.88 years, 6.9% of mean education in this quintile. We can see in the Table 3.2 that there are significant numbers of individuals who smoke at 16 in all of the quintiles thus these results are not due to small numbers of smokers at 16, and the coefficient on smoking at 16 is significant at the 1% level in all quintiles. Far from only affecting the low ability individuals, this evidence indicates that smoking at 16 has a greater absolute and relative effect on the highest ability individuals. This supports the hypothesis that individuals of all abilities smoke at 16 because of their rate of time preference.

To further pursue the hypothesis that individuals who have lower ability are likely to get less education and more likely to smoke, I have replicated my results using smoking at age 18 rather than smoking at age 16. Age 18 is the point at which individuals in the UK have to decide whether to remain in education and go to university, and this decision is likely to be affected by their rate of time preference. Moreover, it is more difficult to argue that smokers at 18 are more likely to be lower ability than higher ability individuals. The right panel of Table 3.2 shows the numbers who smoke at age 18 in the quintiles of the residual log real wage distribution. The Table illustrates that in the lowest quintile the smokers at 18 out number non-smokers (54% v 46%), and this remains the case in the next quintile up (52% smokers v 48% non). As with smoking at 16, the numbers who did smoke are generally lower as we move up the quintiles yet in the highest quintile, still as much as 35% of the individuals smoked at 18. There are a higher number of individuals who smoked at 18 in the upper quintiles than in the corresponding table for smoking at 16, indeed in each quintile there are more smokers at 18 than there were at 16, at least a 10%-point swing to smokers from non-smokers compared with the 16 measure. This further supports the idea that teenage smoking is a habit that high discount rate individuals of all abilities engage in.

Using smoking at 18 as the instrument, I obtain the results in Table 3.9. Looking at the third column, the reduced form equation for schooling, smoking at 18 reduces education by 0.75 years. This is lower than the corresponding reduction associated with smoking at 16 but this is consistent with the time preference story: smokers at 18 have a higher discount rate than *non-smokers* at 18 but *ceteris paribus smokers at 16* will have a higher discount rate than *smokers at 18*. If smokers at 18 have a lower discount rate relative to those who smoke at 16, they will remain in education longer thus we expect that the reduction in education for smoking at 18 is not as much as it is for smoking at 16. The robust standard error on smoking at 18 is 0.108, giving a *t*-statistic with an absolute value of 6.93, therefore the parameter remains precisely estimated. The first stage regression is very similar to first stage regression using smoking when 16. The R^2 for this first stage regression is 0.242 so again high relative to other studies' findings.

Turning to column 2, the estimated return to schooling when we instrument with smoking at 18, is slightly higher at 13.5% than the corresponding figure using smoking at 16 (12.9%), but not by very much. The parameter remains precisely estimated, robust standard error of 0.023 giving a t-statistic of 5.76.

Of the other covariates in the model, each has a coefficient and standard error very close to the estimate when I use smoking at 16, though the R^2 for this new IV estimate is slightly lower at 0.042.

As I get very similar results with smoking at 18 as I do using smoking at 16, and given the distribution of smokers at 16 and 18 throughout the wages distribution, I believe that this is evidence to support the hypothesis that early smoking is picking up the discount rate of the individual rather than being a proxy for ability. Estimates using smoking at 17 rather than 16 or 18 give similar results.

3.8.2 Testing for the discount rate hypothesis

One final test of whether early smoking is picking up differences in time preference is to test whether early smoking is correlated with other future oriented behaviours such as saving, investing and taking precautionary health measures. Home-ownership is one such measure of future orientated behaviour, and Table 3.10 presents a probit of home-ownership in which the explanatory variables are those included in the wage equation (bar years-of-schooling)[23], plus log wage itself and the early smoking indicator. The marginal effects estimated at the means of the explanatory variables suggest that smoking at 16 is associated with a 4.4% reduction in the probability of being a home owner, and is significant at the 1% level. Thus, controlling for human capital and other background characterisitcs to capture heterogeneity, early smoking is associated with a significantly lower probability of being a homeowner, supporting the idea that early smoking is revealing something of the individual's discount rate. There is an obvious

[23] I exclude years-of-schooling, including log wage instead. If years-of-schooling is included it is not significant and alters the smoking coefficient very slightly.

problem in looking at health measures when early smoking is an explanatory variable in that there may be direct consequences of the early smoking on the health outcome, hence the need to look at health related behaviours rather than outcomes. Table 3.11 contains the results of probit regression of having a dental check up in the past year, and having an eye check in the past year, using the same explanatory variables as in the home-ownership probit. Having regular dental and eye check-ups involve trading off future benefits (preventing ill health and associated costs) for current costs (time and expense of appointments) and thus should be influenced by the individual's rate of time preference. As can be seen in these tables, controlling for characteristics and log wage, individuals who were early smokers are 4.0% less likely to have had a dental check up and 2.9% less likely to have had an opticians check up in the past year, each significant at the 1% level. Though these are not perfect indicator measures, with potential problems in each case, they do add to the evidence that the early smoking-education link is capturing the effect of the individual's rate of time preference.

Given all of the tests I have conducted, I am satisfied that smoking at age 16 is a valid instrument for education, and conclude therefore that the OLS estimates are underestimating the return to education. I am not claiming to recover the 'true' return to education and the underlying schooling demand equation. What I have done is estimate the return to education, negating the discount rate bias present in OLS by using smoking at 16 in the schooling equation to generate some variation in schooling which is uncorrelated with the wage equation error term – something that the dual instruments allow me to test (see section (3.10)). Moreover, I am removing the ability bias that is present in OLS estimates, because the instrument is orthogonal to ability – individuals of all abilities can have a high discount rate because of their rate of time preference. (Ability and discount rate are only correlated when we condition on level of schooling.) Therefore I am confident that the instrumental variables estimation has removed the bias from the OLS, allowing a consistent estimate of the return to education.

My estimate ***is*** a local average treatment effect. However, I argue that smoking at 16 demonstrates that the individual has a high discount rate because of their rate of time preference. Thus when I estimate the return to education using smoking at 16 as an instrument, what I am recovering is the average marginal return to education for the group of individuals who have high discount rates not because they have poor access to finance, but because they have a rate of time preference that reflects that they favour the present.

The natural 'local average treatment effect' question is whether I should expect the average marginal return to education to be higher or lower for individuals in this group than the average marginal return to education in the population as a whole? Since individuals of all abilities have rates of time preference that are reflected in a high discount rate, and we have seen that smoking at 16 effects all across the (log wage residual proxying for) ability distribution, we do not have the 'problem' that estimates using compulsory schooling laws are subject to: that they identify returns for individuals with low education and who are (arguably) disproportionately of low ability. If ability is distributed amongst the early smokers group in the same way that it is amongst the population then these early smoker IV estimates are more appropriate for making inferences about the return to education in the population as a whole than similar estimates from IV studies which isolate minimum age school leavers. However that is not to say that estimates derived from the raising of the school leaving age are unsound – only that they are less useful in drawing inference on the average marginal return to education in the population as a whole. What the RoSLA estimates do provide is an estimate of the return to education for those individuals who wanted to leave full-time education at the minimum age – and from a policy point of view this is an important parameter, especially as the Government has recently raised the education leaving age to 17 (from 2013) and it is later to be raised to 18 (by 2015).

The return that I recover is purged of the effects of ability bias and discount rate bias. Both Card (1994, 1998) and Lang (1993) conclude from looking at the broad literature on the effect of ability bias, that ability bias if it is present has only a small biasing effect, Lang suggesting that discount rate bias dominates such that OLS estimates are biased substantially downwards and Card similarly concludes that the OLS are at least 10-to-30% biased downwards. This early smoker IV evidence is consistent with this conclusion – estimating the return to education controlling for ability bias and discount rate bias, we get an estimate that is approximately three times higher than the OLS estimate. If we believe that ability has the same distribution amongst the high discount rate group as it has in the population as a whole, it is more valid to generalise to the population as a whole than perhaps is the case with using estimates recovered from instrumental variables that affect only the low educated.

3.9 Instrumenting Using the Raising of the School Leaving Age (RoSLA)

Now to pursue this line of enquiry further, I will compare the estimate using the early smoking instrument with an IV estimate derived using the raising of the minimum school leaving age. The school leaving age was raised in England and Wales from 15 to 16 in 1973 such that if an individual was 16 by the end of August 1973 he/she was allowed to leave school in the June of 1973, while if the individual was only 15 at the end of August 1973 he/she would have to remain another year at school. This means that those born after August 1957, face a minimum school leaving age of 16. In Scotland this reform took place in August 1976 therefore individuals born after August 1960 face a minimum school leaving age of 16.

This information, plus an individual's date of birth and country of residence, allows the alternative IV estimate to be constructed. Rather than including the smoker at 16 indicator in the first stage regression, I include a dummy to indicate whether the individual faced the minimum school leaving age of 16[24]. As I am controlling for a quadratic in year-of-birth, the smooth changes in schooling as a result of younger cohorts generally gaining more education is controlled for, while the identification derives from the discontinuity induced by the RoSLA. Figure 3.4 shows the proportion of individuals who have left school at or before age 15, by year of birth for the majority of men in my sample[25]. As the figure shows, there is a steady decline in the proportion of men who have left education at 15 or before, and though the relatively small number of men born in any single year in my data means that it is a slightly volatile[26], the pattern of steady decline is evident. In year-of-birth 1958, when the policy is in effect for all individuals, we can see that there is a drop from 17.4% to 1.9% of men leaving at or before 15. The figure remains low for the years thereafter, though with some volatility remaining. Contrasting this is the upper line on the graph which shows the proportion of individuals who have left at age 16 or earlier. While similarly showing a decline as younger cohorts gain more education, the

[24]The minimum school leaving age was raised from 14 to 15, in 1947 for England and Wales, 1946 for Scotland, however, in the sample of men that I use, there are only 73 individuals (3.22%) who face a minimum school leaving age of 14 so I have concentrated on the later change to create an instrument.

[25]I have trimmed the sample to remove the small number of men born before 1931 and after 1970 due to the small cell sizes, the graph contains the information for 83.9% of the English men in the sample. I have excluded the small number of Scottish men for the purpose of this illustration as the RoSLA occured later for Scotland.

[26]As year-of-birth increases the cell sizes increase and for the years relevant to the RoSLA the numbers are larger.

proportion who have left by or at 16 continues after the RoSLA to show volatility, rising and falling quite sharply in places. So while the small numbers of men born in any particular year lead to volatility in each graph, it is evident that the RoSLA results in a discontinuity at the point in which is was implemented, and it is from this discontinuity that I am able to construct the IV estimates using RoSLA.

Table 3.12 contains the results for the RoSLA IV along with the OLS estimates (from Table 3.5). Column 1 contains the OLS results, column 2 is the result from the IV using smoking at 16, while column 3 contains the first stage regression result using the raising of the school leaving age as the instrument.

The main columns of interest are columns 2 and 3. Looking first at column 3, the raising of the school leaving age is associated with an increase in education of 0.564 years and the coefficient is precisely estimated with a robust standard error of 0.206 giving a t-statistic of 2.74. Again, it is noticeable that the R^2 (0.227) is higher than has been found in similar studies. The partial-R^2 for the instrument in the first stage is 0.0044 which is smaller than for the early smoker instrument but is exactly the same as that found by Harmon and Walker (1995) for their first stage, and compares well with Bound *et al.* (1995). The F-statistics on instrument is 7.49 which is below Staiger and Stock's (1997) rule-of-thumb guide of 10, though taken with the partial R^2, the overall picture is not of a weak instrument. Moreover, the R^2 and the F together again suggest that the bias inherent in IV in finite samples will be lower than the OLS bias. The size of the average increase in education, controlling for other covariates in the first stage, is comparable with that found by Harmon and Walker (1995) (0.54 years for the 1947 RoSLA), and slightly larger than that found by Oreopoulos using just the 1947 RoSLA (0.44 years).

Turning to column 2, we see that the estimated return to schooling is 10.2% when we instrument using RoSLA. This is more than double the size of the OLS return though below the other IV estimate. However it is not as precisely estimated, the robust standard error is 0.051 giving a t-statistic of 1.99, the p-value of this t-statistic is 0.046 therefore it is significant at the 5% level.

Again, as a robustness check to verify that the inclusion of the parental characteristics variables are not driving the result, Table 3.13 displays the results for the more basic specification excluding these background variables. In this more basic specification, the instrument is actually strengthened, the F-statistics on the exclusion of the instrument from the first stage increasing to 9.98 (much closer to Staiger and Stock's rule-of-thumb of 10) and the partial R^2 of the instrument is 0.0058 (increased from 0.0044 in the main specification), and the overall first stage R^2 is 0.113. The effect on the estimated return to education is minor – reducing from 10.2% to 10.0%, with a robust standard error of 0.042 giving a t-statistic of 2.41, making the estimate signficant at the 5% level (p-value 0.016). Thus again the inclusion of parental characteristic variables is not driving the result.

The question is whether this is evidence that using an institutional change – such as the raising of the school leaving age to form an instrument – isolates the return to schooling for only a specific group that is heavily weighted towards the low ability and those with high discount rate particularly because of financial constraints?

If the group whose return is identified by the RoSLA instrument (which is by definition a low education group) is comprised mainly of individuals of low ability rather than those who have high discount rates because of poor access to finance, then we would expect that the return for this group would be lower than the return we find with the smoker at 16 instrument – as I have demonstrated that individuals of all abilities are in the early smokers group. The imprecision of the estimate using RoSLA does not allow me to conclude that the estimate is definitely smaller than the smoking at 16 IV estimate, however one test of the extent to which RoSLA affects individuals of different abilities is to repeat the first stage regressions by quintile of the residual log wage distribution that I used to illustrate the effect of smoking at 16 on educational attainment in all quintiles of the distribution. The results from these regressions are in right hand section of Table 3.8. If the contention is that RoSLA affects primarily low ability individuals then we would expect that the effect would be quantitatively larger for the lowest quintiles of the residual log wage distribution but falling in size and significance as we move up the distribution.

Table 3.8 illustrates that the raising of the minimum school leaving age increases the number of years of schooling by 1.04 years in the lowest quintile, which is 8.4% of the mean number of years schooling for this group. Being almost exactly 1 year extra education this suggests that in this lower quintile of the (proxy) ability distribution, all the individuals wished to leave school at the minimum age. In the second lowest quintile RoSLA increases the number of years of schooling by 0.84 years which is 6.9% of the mean for this group. In the three quintiles above this the increase in education associated with RoSLA is much smaller in absolute and relative terms than in both of the lowest two quintiles but in none of these higher quintiles is the dummy for minimum school leaving age of 16 close to being statistically significant.

This evidence is consistent with the hypothesis that the low education group affected by RoSLA are generally lower ability – if they were mainly high discount rate then we would expect to see a similar effect across the residual log wage distribution.

The contention that the RoSLA group is weighted more towards low ability rather than high discount rate individuals is supported by Carneiro and Heckman (2002). They find that in the US, only 8% of American youths are credit constrained to the point that it affects their post-secondary schooling. Moreover, they find that when ability is controlled for responses to tuition costs are uniform across income groups. Low family income at the time when decisions over post-secondary education are made does not appear to be a major constraint in the US. Two recent studies in the UK have indicated that credit constraints do not prevent individuals from participating in higher education. Chowdry *et al.* (2008) use a unique dataset from a cohort comprising all state school pupils who were in the final year of compulsory schooling in England in 2001-2002. These students have been followed from age 11 through to their higher education participation decision at age 18 (in 2004-05) or age 19 (2005-06). The results indicate that conditional on prior attainment, there is no difference in higher (university) education participation rates between children of higher and lower socio-economic status (SES) – illustrating for the UK, what Carneiro and Heckman find for the US. Similarly, Wyness (2008) studies the effect of alterations to the funding of higher education in England – with the introduction of fees and indeed top-up fees. Wyness finds that participation rates among the lower SES groups have not declined following the introduction of tuition fees, which again supports the contention that the RoSLA group in this country are not credit constrained.

If it is the case that those affected by RoSLA are high discount rate rather than low ability, the IV results which use RoSLA may well be higher than the OLS estimates. However, the evidence above and these conclusions from the Carneiro and Heckman, Chowdry *et al.* and Wyness papers suggest that it is more likely to be the case that the group identified by RoSLA are individuals of low ability rather than high discount rate. Comparing the RoSLA IV results with the early smoking IV results would support the idea that the RoSLA group are low ability as the RoSLA IV estimates a lower return, though the imprecision of the IV estimates prevents a concrete conclusion that the estimate is indeed lower using RoSLA.

3.10 Testing of the Instruments

Having more than one instrument means that I have an over-identified system – more moment conditions than are necessary to identify the parameters of the model – which means that I can test the instruments to establish whether the exclusion restrictions are valid. In other studies, such as Angrist and Krueger (1991) and Evans and Montgomery (1994), multiple instruments are used and tested. In each of these cases however, they essentially only have one mechanism to generate the exogenous variation in education: including interactions of that mechanism (the instrument) with other variables does not entail genuinely having multiple instruments. If the mechanism is not valid then none of the 'instruments' are valid, the problem being that the Hansen J-test of the exclusion restrictions involves assuming one of the instruments is valid in order to test the others.

On the contrary, I have two independent sources of exogenous variation in education and so can genuinely test the validity of the exclusion restrictions. As Murray (2006) points out, the Hansen test is more compelling when one of the instruments is thought to be definitely valid, and I believe that I am in this situation: there is a strong argument to suggest that the RoSLA instrument is valid as it was an exogenous (to the individual) policy change.

Instrumenting using both the early smoking instrument and the minimum school leaving age instrument and then performing the Hansen J-test results in a test statistic of 0.202, p-value 0.6529, which is a comprehensive failure to reject the null hypothesis that the instruments are valid. The first stage R^2 is high at 0.250 and the F-statistics on the exclusion of the instruments is 36.83 with a partial R^2 on the instruments of 0.0332, all of which suggests that the instruments are both strong and valid.

As can be seen in Table 3.14, the coefficient on each instrument in the first stage is almost identical to the case when the instruments are used separately, and the estimated return to education using both instruments together is 12.5% with a robust standard error of 0.019 giving a t-statistic of 6.66. The standard error is lower than is the case when either of the instruments are used singly, so the extra variation in schooling that comes with using both instruments results in a more precise estimate of the IV return to education.

The problem with this strategy is that using both instruments makes the interpretation 'ugly', to borrow Murray's parlance. Though I am exploiting two sources of exogenous variation in years-of-schooling, which is good for identification, the problem is interpreting exactly whose return the resulting LATE estimator is capturing. It is not as straightforward as in the individual instruments cases in which we identify the low ability/high discount rate individuals' return – using RoSLA – or the high discount rate (because of time preference) individuals' return – using early smoking. Given that the effects of each instrument in the first stage are similar to their impacts when used separately, and that the early smoking instrument is the stronger and the resulting IV estimate of the return is very close to the early smoking IV estimate, it appears that this instrument is doing most of the work. In interpretation this would suggest the estimate is more heavily weighted towards the return for the individuals who have high discount rates because of their rate of time preference.

3.11 Conclusions

I have presented three IV estimates: the RoSLA estimate of 10.2%, the combined estimate of 12.5%, and the early smoking estimate of 12.9%, all of which whilst being statistically significant are sufficiently imprecise for me to be unable to conclude are actually different from each other. My analysis, looking at the effects on different quintiles of the proxy ability distribution, suggests that the RoSLA estimate captures the return for the individuals who wanted to leave at the minimum leaving age but were forced to stay longer – concurring with the earlier evidence of Oreopoulos, Chevalier *et al.* and Harmon and Walker. I have argued that early smoking is a behaviour engaged in by individuals of all abilities who have high discount rates due to their rate of time preference, thus the IV estimate derived from this instrument is closer to an average marginal return to education, purged of the bias of OLS. That both the RoSLA and early smoking IV estimates are not statistically different to each other suggests that the RoSLA LATE is also close to an average marginal return to education i.e. that the returns at the lower part of the distribution are similar to the average return. This follows Oreopoulos who finds a return substantially higher than the estimated OLS return when implementing IV esitmates based on RoSLA, and a RoSLA that affected a large proportion of the population.

I believe that the results that this and other IV studies find can be reconciled when we consider the assumptions imposed by Mincer's human capital earnings function as I (and others) have estimated it. Implicit in this specification is the assumption that each additional year of schooling has the same proportional effect on earnings i.e. concavity in the schooling-wage profile is not modelled. Moreover, in interpreting IV estimates we need explicitly recognise that returns to education vary across the population depending on characteristics such as family background and ability (the β_i vary). If different individuals have different returns to schooling at the same level of schooling and if each individual's return to schooling is strictly decreasing in their level of schooling, then there is no **unique** causal effect of schooling.

While some authors[27] have concentrated on "sheep-skin" effects creating non-linearities in the returns to education, Lang (1993) finds a diminishing marginal product of education i.e. concavity in the education-wage profile. The individuals affected by RoSLA may be of lower ability, however, if all individuals have a higher marginal return to schooling at lower levels of schooling then this is consistent with the estimate from the RoSLA IV being higher than the OLS estimate. The early smoking instrument estimates the return for individuals who have a higher than average discount rate, and therefore will have a higher than average return at the point at which they have stopped acquiring more education. Therefore in this case it is not a surprise that the estimated return is higher than the OLS. Moreover, the smoking at 16 group have all levels of education, some higher than the minimum that the RoSLA individuals have by definition, though there is more weight in the lower part of the schooling distribution among early smokers and so the the average marginal return across these individuals will be close to the RoSLA estimate. Thus in this light it is reasonable that both the smoking instrument and the RoSLA instrument result in estimates of the return to education that are similar to each other.

More generally there is a question as to why the OLS estimates are consistently found to be below IV estimates – irrespective of the instrument chosen – when, as noted above, measurement

[27] For example, Park (1999) has looked at "sheep-skin" effects in the US.

error in standard micro surveys could only sensibly account for a relatively small attenuation in the OLS coefficient and moreover it appears from this study that 'discount rate bias' is not a major factor biasing the OLS estimates downwards. The 'discount rate bias' story suggests that the effect of discount rate to reduce education also independently increases wages. However, when I test for the correlation between the discount rate (as captured by early smoking) and the wage error the instrument is shown to be valid. Hence I do not believe that 'discount rate bias' is the major factor biasing the OLS estimates downwards. Given that all instruments estimate a 'local average treatment effect', which may or may not be different to the average effect on the treated, it appears that the instruments that have commonly been used – and the two that I use here – isolate the treatment effect for groups of individuals who have a higher average return to education at the point in the education distribution at which the instrument works, than the global average estimated by OLS. Support for this conclusion also comes from Oreopoulos (2006) who estimates that when the OLS is carried out only for those who left school at 16 or less, the estimated coefficient is similar to his IV estimates which use RoSLA. If I replicate this approach and estimate the OLS regression only for those who left school at the minimum age the estimated return is 19.7%. Whilst acknowledging that the endogeneity of years of schooling in this regression is not dealt with, the much greater coefficient on years of schooling does suggest that the linearity in returns assumption of the OLS esimated over the entire range of education levels contributes significantly to the lowering of the OLS coefficient.

One conclusion is that in modelling the returns to education, while the endogeneity of schooling is clearly a problem, it is important to recognize that there are also issues regarding the appropriateness of the linearity assumption and the reality of heterogeneous returns to education across individuals. Thus for policy purposes in particular, it may not even be appropriate to refer to ***the*** causal effect of education on earnings. In answering the question of the return, we may need to focus on the individuals in question and the margin in question before we can arrive at a valid answer.

3.12 Tables

Table 3.1: Effect of schooling on probability of Current and Early Smoking

	Current Smoker		**Smoker at 16**		
	marginal fx	**z**	**marginal fx**	**z**	**x-bar**
years of schooling	-0.027***	-7.24	-0.038***	-8.38	12.306
age	0.005	1.35	0.002	0.50	42.374
age^2	0.000***	-2.99	0.000	-0.36	1939.210
year-of-birth	-0.016***	-2.66	-0.002	-0.30	59.190 (=1955)
year-of-birth^2	0.000***	2.84	0.000	0.25	3647.300
region: North	-0.032	-0.76	-0.033	-0.66	0.066
region: Yorkshire	0.037	0.99	0.031	0.69	0.098
region: North West	0.037	0.97	0.031	0.69	0.104
region: East Midlands	0.056	1.46	0.060	1.30	0.094
region: East Anglia	0.047	1.05	0.112*	1.94	0.043
region: South East	0.055*	1.72	0.047	1.23	0.285
region: South West	-0.005	-0.12	0.059	1.29	0.097
region: Wales	0.062	1.36	0.006	0.11	0.053
region: Scotland	0.065	1.57	0.063	1.28	0.078
ethnicity: black	-0.121	-1.27	-0.181	-1.39	0.006
ethnicity: asian	0.225***	3.18	-0.176**	-2.29	0.016
ethnicity: other	-0.016	-0.16	-0.174	-1.55	0.008
father's occ class: 1	-0.022	-0.74	-0.041	-1.17	0.141
father's occ class: 2	-0.094**	-2.39	-0.094*	-1.88	0.058
father's occ class: 3	0.013	0.28	-0.099*	-1.81	0.035
father's occ class: 4	-0.055	-1.28	-0.104**	-2.10	0.047
father's occ class: 5	0.012	0.46	-0.010	-0.33	0.236
father's occ class: 6	0.022	0.49	-0.089*	-1.79	0.042
father's occ class: 7	0.006	0.13	-0.002	-0.03	0.032
father's occ class: 9	0.009	0.29	0.016	0.43	0.094
father's occ class: 10	-0.012	-0.39	-0.056*	-1.67	0.151
mother's occ class: 1	-0.017	-0.33	-0.026	-0.39	0.037
mother's occ class: 2	0.070	1.07	0.003	0.04	0.027
mother's occ class: 3	-0.050	-0.87	-0.036	-0.51	0.030
mother's occ class: 4	-0.036	-0.84	-0.023	-0.42	0.089
mother's occ class: 5	-0.023	-0.41	0.048	0.67	0.028
mother's occ class: 6	0.025	0.51	0.036	0.62	0.068
mother's occ class: 7	-0.054	-1.16	-0.083	-1.48	0.060
mother's occ class: 9	-0.017	-0.39	-0.057	-1.08	0.083
mother's occ class: 10	-0.012	-0.33	-0.060	-1.29	0.532
'nuclear family' to 16	-0.062***	-2.86	-0.099***	-3.73	0.820
mid 1990s	0.002	0.22	0.000	0.06	0.223
late 1990s	0.037***	2.90	0.035***	2.85	0.200
post 2000	-0.006	-0.30	0.002	0.08	0.371
# individuals	2805		2805		
# observations	33298		33298		
obs. prob.	0.287		0.344		
pred. prob. (at x-bar)	0.276		0.331		

Notes: Reference categories: West Midlands, white, did not live with both natutal parents to 16, father/mother occupational class 'plant/machine operative'. Occupational Class dummies: (1) management, (2) professional, (3) associate professional/technical, (4) clerical/secretarial, (5) craft and related, (6) personal/protective services, (7) sales, (9) other, (10) self-emp/unemp.

Table 3.2: Smokers at 16/18 by quintile of the mean residual log wage distribution

quintile	**Non-smoker at 16**	**Smoker at 16**	**Total**	**Non-smoker at 18**	**Smoker at 18**	**Total**
1	256	198	454	209	245	454
	56.39%	43.61%	100.00%	46.04%	53.96%	100.00%
2	278	175	453	216	237	453
	61.37%	38.63%	100.00%	47.68%	52.32%	100.00%
3	319	134	453	265	188	453
	70.42%	29.58%	100.00%	58.50%	41.50%	100.00%
4	299	154	453	255	198	453
	66.00%	34.00%	100.00%	56.29%	43.71%	100.00%
5	349	104	453	295	158	453
	77.04%	22.96%	100.00%	65.12%	34.88%	100.00%
Total	1501	765	2266	1240	1026	2266
	66.24%	33.76%	100.00%	54.72%	45.28%	100.00%

Notes: OLS log wage regression (Table 3.5 column 1) run on pooled panel dataset, residuals are taken and the mean residual for each individual is calculated. These are then ranked into 5 quintiles as a measure of unobserved ability.

Table 3.3: Sample Summary Statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
log wage	21256	2.214	0.437	0.848	3.407
years of schooling	21256	12.507	2.646	7	21
smoker at age 16	21256	0.315	0.465	0	1
minimum school leaving age was 16	21256	0.537	0.499	0	1
age	21256	39.991	10.757	19	65
cohort: born in the 1920s	21256	0.003	0.052	0	1
cohort: born in the 1930s	21256	0.050	0.219	0	1
cohort: born in the 1940s	21256	0.203	0.402	0	1
cohort: born in the 1950s	21256	0.274	0.446	0	1
cohort: born in the 1960s	21256	0.319	0.466	0	1
cohort: born in the 1970s	21256	0.146	0.354	0	1
cohort: born in the 1980s	21256	0.005	0.069	0	1
region: North	21256	0.068	0.252	0	1
region: Yorkshire	21256	0.097	0.296	0	1
region: North West	21256	0.106	0.308	0	1
region: East Midlands	21256	0.092	0.290	0	1
region: East Anglia	21256	0.043	0.202	0	1
region: South East	21256	0.280	0.449	0	1
region: South West	21256	0.100	0.300	0	1
region: Wales	21256	0.051	0.221	0	1
region: Scotland	21256	0.076	0.265	0	1
ethnicity: black	21256	0.004	0.062	0	1
ethnicity: asian	21256	0.016	0.124	0	1
ethnicity: other	21256	0.007	0.083	0	1
father's occ class: 1	21256	0.139	0.346	0	1
father's occ class: 2	21256	0.064	0.244	0	1
father's occ class: 3	21256	0.038	0.191	0	1
father's occ class: 4	21256	0.049	0.216	0	1
father's occ class: 5	21256	0.234	0.423	0	1
father's occ class: 6	21256	0.044	0.205	0	1
father's occ class: 7	21256	0.032	0.177	0	1
father's occ class: 8	21256	0.171	0.377	0	1
father's occ class: 9	21256	0.086	0.280	0	1
father's occ class: 10	21256	0.143	0.350	0	1
mother's occ class: 1	21256	0.037	0.188	0	1
mother's occ class: 2	21256	0.026	0.159	0	1
mother's occ class: 3	21256	0.032	0.175	0	1
mother's occ class: 4	21256	0.098	0.297	0	1
mother's occ class: 5	21256	0.029	0.168	0	1
mother's occ class: 6	21256	0.073	0.260	0	1
mother's occ class: 7	21256	0.066	0.248	0	1
mother's occ class: 8	21256	0.051	0.220	0	1
mother's occ class: 9	21256	0.084	0.277	0	1
mother's occ class: 10	21256	0.505	0.500	0	1
'nuclear family' to 16	21256	0.831	0.375	0	1
early 1990s	21256	0.195	0.396	0	1
mid 1990s	21256	0.213	0.409	0	1
late 1990s	21256	0.221	0.415	0	1
post 2000	21256	0.371	0.483	0	1
number of observations per person	2266	9.380	4.516	1	15

Notes: 'nuclear family' means living with both natural parents
Occupational class dummies: (1) management, (2) professional, (3) associate professional/technical, (4) clerical/secretarial, (5) craft and related, (6) personal/protective services, (7) sales, (8) plant/machine operative, (9) other, (10) self-emp/unemp.

Table 3.4: Sample Summary Statistics, by Early Smoking Status

	Smoker at 16					Non-Smoker at 16				
Variable	**Obs**	**Mean**	**Std. Dev.**	**Min**	**Max**	**Obs**	**Mean**	**Std. Dev.**	**Min**	**Max**
log wage	6696	2.120	0.423	0.878	3.395	14560	2.257	0.437	0.848	3.407
years of schooling	6696	11.646	2.081	8	21	14560	12.902	2.781	7	21
smoker at age 16	6696	1.000	0.000	1	1	14560	0.000	0.000	0	0
min. sch leaving age 16	6696	0.486	0.500	0	1	14560	0.561	0.496	0	1
age	6696	41.230	11.337	19	65	14560	39.421	10.431	19	65
cohort: born in the 1920s	6696	0.004	0.063	0	1	14560	0.002	0.046	0	1
cohort: born in the 1930s	6696	0.063	0.244	0	1	14560	0.044	0.206	0	1
cohort: born in the 1940s	6696	0.258	0.438	0	1	14560	0.177	0.382	0	1
cohort: born in the 1950s	6696	0.250	0.433	0	1	14560	0.284	0.451	0	1
cohort: born in the 1960s	6696	0.266	0.442	0	1	14560	0.344	0.475	0	1
cohort: born in the 1970s	6696	0.150	0.357	0	1	14560	0.145	0.352	0	1
cohort: born in the 1980s	6696	0.008	0.091	0	1	14560	0.003	0.057	0	1
region: North	6696	0.057	0.231	0	1	14560	0.073	0.260	0	1
region: Yorkshire	6696	0.103	0.304	0	1	14560	0.095	0.293	0	1
region: North West	6696	0.103	0.303	0	1	14560	0.107	0.309	0	1
region: East Midlands	6696	0.095	0.293	0	1	14560	0.091	0.288	0	1
region: East Anglia	6696	0.050	0.219	0	1	14560	0.039	0.193	0	1
region: South East	6696	0.272	0.445	0	1	14560	0.284	0.451	0	1
region: South West	6696	0.119	0.324	0	1	14560	0.091	0.288	0	1
region: Wales	6696	0.044	0.204	0	1	14560	0.055	0.228	0	1
region: Scotland	6696	0.082	0.274	0	1	14560	0.074	0.261	0	1
ethnicity: black	6696	0.001	0.037	0	1	14560	0.005	0.070	0	1
ethnicity: asian	6696	0.006	0.075	0	1	14560	0.020	0.140	0	1
ethnicity: other	6696	0.002	0.049	0	1	14560	0.009	0.094	0	1
father's occ class: 1	6696	0.123	0.329	0	1	14560	0.146	0.353	0	1
father's occ class: 2	6696	0.036	0.185	0	1	14560	0.077	0.266	0	1
father's occ class: 3	6696	0.027	0.161	0	1	14560	0.043	0.203	0	1
father's occ class: 4	6696	0.032	0.177	0	1	14560	0.057	0.231	0	1
father's occ class: 5	6696	0.257	0.437	0	1	14560	0.223	0.416	0	1
father's occ class: 6	6696	0.036	0.187	0	1	14560	0.048	0.213	0	1
father's occ class: 7	6696	0.036	0.187	0	1	14560	0.031	0.172	0	1
father's occ class: 8	6696	0.194	0.396	0	1	14560	0.161	0.367	0	1
father's occ class: 9	6696	0.113	0.316	0	1	14560	0.074	0.261	0	1
father's occ class: 10	6696	0.146	0.353	0	1	14560	0.142	0.349	0	1
mother's occ class: 1	6696	0.031	0.173	0	1	14560	0.039	0.195	0	1
mother's occ class: 2	6696	0.018	0.134	0	1	14560	0.030	0.169	0	1
mother's occ class: 3	6696	0.028	0.166	0	1	14560	0.033	0.179	0	1
mother's occ class: 4	6696	0.085	0.279	0	1	14560	0.104	0.305	0	1
mother's occ class: 5	6696	0.043	0.204	0	1	14560	0.023	0.148	0	1
mother's occ class: 6	6696	0.098	0.297	0	1	14560	0.062	0.240	0	1
mother's occ class: 7	6696	0.051	0.221	0	1	14560	0.072	0.259	0	1
mother's occ class: 8	6696	0.053	0.223	0	1	14560	0.050	0.218	0	1
mother's occ class: 9	6696	0.092	0.288	0	1	14560	0.080	0.272	0	1
mother's occ class: 10	6696	0.501	0.500	0	1	14560	0.507	0.500	0	1
'nuclear family' to 16	6696	0.795	0.404	0	1	14560	0.848	0.359	0	1
early 1990s	6696	0.196	0.397	0	1	14560	0.194	0.396	0	1
mid 1990s	6696	0.212	0.409	0	1	14560	0.214	0.410	0	1
late 1990s	6696	0.219	0.414	0	1	14560	0.222	0.415	0	1
post 2000	6696	0.373	0.484	0	1	14560	0.371	0.483	0	1
# obs. per person	765	8.753	4.650	1	15	1501	9.700	4.414	1	15

Notes: 'nuclear family' means living with both natural parents
Occupational class dummies: (1) management, (2) professional, (3) associate professional/technical, (4) clerical/secretarial, (5) craft and related, (6) personal/protective services, (7) sales, (8) plant/machine operative, (9) other, (10) self-emp/unemp.

Table 3.5: Human Capital Earnings Function Estimations, OLS and IV using Smoker at 16 Status

	OLS		IV: smoker at 16		IV: first stage	
Dep. Var: log hourly wage	**Coeff.**	**Robust Std. Err.**	**Coeff.**	**Robust Std. Err.**	**Coeff.**	**Robust Std. Err.**
constant	-0.754***	0.250	-0.607**	0.287	-0.471	1.664
years of schooling	0.046***	0.003	0.129***	0.020	—	—
smoker at 16 indicator	—	—	—	—	-0.876***	0.108
age	0.099***	0.004	0.094***	0.005	0.056***	0.022
age^2	-0.001***	0.000	-0.001***	0.000	-0.001***	0.000
year-of-birth	-0.016**	0.007	-0.052***	0.011	0.398***	0.041
year-of-birth^2	0.000***	0.000	0.001***	0.000	-0.003***	0.000
region: North	0.047	0.038	0.054	0.044	-0.103	0.272
region: Yorkshire	0.003	0.033	-0.022	0.041	0.331	0.253
region: North West	0.054	0.032	0.023	0.040	0.402	0.253
region: East Midlands	-0.010	0.032	-0.005	0.038	-0.034	0.235
region: East Anglia	0.015	0.039	-0.009	0.048	0.366	0.324
region: South East	0.142***	0.028	0.082**	0.037	0.757***	0.206
region: South West	0.023	0.034	0.015	0.041	0.175	0.237
region: Wales	-0.012	0.040	-0.019	0.045	0.081	0.285
region: Scotland	0.028	0.036	-0.021	0.044	0.643**	0.262
ethnicity: black	0.114	0.105	0.115	0.117	-0.164	0.779
ethnicity: asian	-0.136*	0.071	-0.312***	0.105	1.965***	0.485
ethnicity: other	-0.048	0.103	-0.234**	0.119	2.067*	1.111
father's occ class: 1	0.116***	0.028	0.020	0.041	1.122***	0.214
father's occ class: 2	0.121***	0.038	-0.077	0.065	2.268***	0.291
father's occ class: 3	0.089**	0.043	-0.043	0.058	1.499***	0.321
father's occ class: 4	0.065*	0.036	-0.053	0.051	1.320***	0.305
father's occ class: 5	0.038*	0.023	0.011	0.028	0.335**	0.170
father's occ class: 6	0.014	0.035	-0.074	0.048	0.991***	0.305
father's occ class: 7	0.103***	0.040	0.066	0.049	0.467	0.330
father's occ class: 9	-0.021	0.029	0.028	0.035	-0.551***	0.197
father's occ class: 10	0.029	0.027	0.027	0.030	-0.012	0.186
mother's occ class: 1	0.047	0.049	0.035	0.061	0.112	0.411
mother's occ class: 2	0.015	0.054	-0.103	0.070	1.433***	0.439
mother's occ class: 3	0.056	0.048	0.053	0.057	0.046	0.387
mother's occ class: 4	0.055	0.040	0.014	0.048	0.485	0.307
mother's occ class: 5	0.010	0.049	0.031	0.058	-0.117	0.417
mother's occ class: 6	0.025	0.040	0.029	0.045	0.054	0.311
mother's occ class: 7	0.055	0.041	0.057	0.048	-0.083	0.312
mother's occ class: 9	-0.004	0.038	0.034	0.044	-0.461	0.284
mother's occ class: 10	0.004	0.032	-0.006	0.036	0.115	0.253
'nuclear family' to 16	0.028	0.019	0.001	0.022	0.247*	0.136
mid 1990s	-0.045***	0.009	-0.050***	0.010	0.067	0.046
late 1990s	-0.065***	0.014	-0.070***	0.016	0.080	0.081
post 2000	-0.033	0.021	-0.040*	0.023	0.108	0.126
# observations	21256		21256		21256	
# individuals	2266		2266		2266	
R^2	0.265		0.072		0.246	
F-test on exclusion of smoking at 16 from first stage: 66.17; Partial R^2 of instrument = 0.0289						

Notes: *** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level
Standard errors are clustered at the level of the individual and robust.
'nuclear family' to 16 means lived with both natural parents from birth to age 16.
Reference categories: West Midlands, white, did not live with both natutal parents to 16, father/mother occupational class 'plant/machine operative'. Occupational Class dummies: (1) management, (2) professional, (3) associate professional/technical, (4) clerical/secretarial, (5) craft and related, (6) personal/protective services, (7) sales, (9) other, (10) self-emp/unemp.

Table 3.6: Human Capital Earnings Function Estimations, OLS and IV using Smoker at 16 Status, Basic Specification

	OLS		**IV: smoker at 16**		**IV: first stage**	
Dep. Var: log hourly wage	**Coeff.**	**Robust Std. Err.**	**Coeff.**	**Robust Std. Err.**	**Coeff.**	**Robust Std. Err.**
constant	-0.849***	0.247	-0.590**	0.280	-2.204	1.697
years of schooling	0.052***	0.003	0.121***	0.016		
smoker at 16 indicator					-1.087***	0.113
age	0.098***	0.004	0.094***	0.005	0.067***	0.023
age^2	-0.001***	0.000	-0.001***	0.000	-0.001***	0.000
year-of-birth	-0.014**	0.007	-0.048***	0.011	0.466***	0.042
year-of-birth^2	0.000***	0.000	0.001***	0.000	-0.004	0.000
region: North	0.041	0.038	0.048	0.044	-0.124	0.286
region: Yorkshire	-0.003	0.033	-0.018	0.039	0.269	0.265
region: North West	0.050	0.033	0.022	0.039	0.432	0.270
region: East Midlands	-0.016	0.032	-0.006	0.037	-0.122	0.258
region: East Anglia	0.010	0.040	-0.006	0.047	0.318	0.338
region: South East	0.143***	0.028	0.080***	0.036	0.946***	0.219
region: South West	0.023	0.034	0.010	0.040	0.271	0.251
region: Wales	-0.018	0.040	-0.019	0.044	0.003	0.305
region: Scotland	0.020	0.036	-0.023	0.043	0.688**	0.283
ethnicity: black	0.117	0.093	0.104	0.113	-0.015	0.751
ethnicity: asian	-0.150**	0.070	-0.290***	0.098	1.844***	0.519
ethnicity: other	-0.042	0.095	-0.221*	0.119	2.406**	0.996
mid 1990s	-0.047***	0.009	-0.049***	0.010	0.040	0.050
late 1990s	-0.068***	0.014	-0.068***	0.016	0.019	0.086
post 2000	-0.038*	0.021	-0.037	0.023	0.014	0.133
# observations	21256		21256		21256	
# individuals	2266		2266		2266	
R^2	0.251		0.098		0.143	
F-test on exclusion of instrument from first stage: 92.39; Partial R^2 of the instrument = 0.0400						

Notes: *** significant at 1% level, ** significant at 5% level, * significant at 10% level.
Standard errors are clustered at the level of the individual and robust.

Table 3.7: First Stage IV regression coefficients using Smoker at 16 indicator interacted with year turned 16 indicator

	Coeff.	Robust Std. Err.	t	p
Smoker at 16 x turned 16 pre-1965	-0.797***	0.209	-3.82	0.000
Smoker at 16 x turned 16 post-1965	-0.904***	0.120	-7.51	0.000
# observations	21256			
R^2	0.247			

Notes: *** significant at 1% level; standard errors clustered at individual level and robust.
Turned 16 post-1965 includes those turning 16 from January 1965 onwards.
Other covariates included in these first stage regressions are those in Table 3.5.

Table 3.8: First Stage IV Regression coefficients on Smoker at 16 indicator and on Minimum School Leaving Age of 16 indicator, by quintile of the mean residual log wage distribution

	IV first stage, Early Smoking			IV first stage, RoSLA		
quintile	Coeff. on smoker 16	Robust Std. Err.	R^2	Coeff. on MSLA=16	Robust Std. Err.	R^2
1 #obs = 3684 mean years of schooling 12.41	-0.773***	0.265	0.268	1.044**	0.510	0.262
2 #obs = 4285 mean years of schooling 12.09	-1.044***	0.227	0.317	0.837*	0.458	0.292
3 #obs = 4461 mean years of schooling 12.30	-0.950***	0.249	0.329	0.315	0.496	0.309
4 #obs = 4496 mean years of schooling 12.28	-0.747***	0.213	0.257	0.398	0.388	0.240
5 #obs = 4330 mean years of schooling 12.65	-0.879***	0.241	0.341	0.080	0.435	0.321

Notes: *** signficant at 1% level, ** significant at 5% level, * significant at 10% level
Standard errors are clustered at the level of the individual and robust.
Other covariates included in regressions are as Table 3.5.

Table 3.9: Human Capital Earnings Function Estimations, OLS and IV using Smoker at 18 Status

	OLS		IV: smoker at 18		IV: first stage	
Dep. Var: log hourly wage	**Coeff.**	**Robust Std. Err.**	**Coeff.**	**Robust Std. Err.**	**Coeff.**	**Robust Std. Err.**
constant	-0.754***	0.250	-0.596**	0.293	-0.399	1.675
years of schooling	0.046***	0.003	0.135***	0.023	—	—
smoker at 18 indicator	—	—	—	—	-0.745***	0.108
age	0.099***	0.004	0.093***	0.005	0.054**	0.022
age^2	-0.001***	0.000	-0.001***	0.000	-0.001***	0.000
year-of-birth	-0.016**	0.007	-0.054***	0.012	0.399***	0.041
year-of-birth^2	0.000***	0.000	0.001***	0.000	-0.003***	0.000
region: North	0.047	0.038	0.054	0.045	-0.121	0.274
region: Yorkshire	0.003	0.033	-0.024	0.042	0.319	0.254
region: North West	0.054	0.032	0.020	0.041	0.414	0.253
region: East Midlands	-0.010	0.032	-0.005	0.038	-0.041	0.236
region: East Anglia	0.015	0.039	-0.011	0.050	0.392	0.324
region: South East	0.142***	0.028	0.077**	0.039	0.760***	0.209
region: South West	0.023	0.034	0.014	0.042	0.138	0.238
region: Wales	-0.012	0.040	-0.020	0.046	0.063	0.287
region: Scotland	0.028	0.036	-0.025	0.046	0.600**	0.264
ethnicity: black	0.114	0.105	0.115	0.119	-0.212	0.774
ethnicity: asian	-0.136	0.071	-0.325***	0.112	2.081***	0.511
ethnicity: other	-0.048	0.103	-0.248**	0.124	2.112*	1.091
father's occ class: 1	0.116***	0.028	0.013	0.044	1.160***	0.213
father's occ class: 2	0.121***	0.038	-0.092	0.071	2.327***	0.292
father's occ class: 3	0.089**	0.043	-0.053	0.063	1.514***	0.326
father's occ class: 4	0.065*	0.036	-0.062	0.054	1.362***	0.309
father's occ class: 5	0.038*	0.023	0.009	0.029	0.340**	0.170
father's occ class: 6	0.014	0.035	-0.081	0.051	0.983***	0.308
father's occ class: 7	0.103***	0.040	0.063	0.050	0.493	0.329
father's occ class: 9	-0.021	0.029	0.032	0.036	-0.551***	0.196
father's occ class: 10	0.029	0.027	0.026	0.031	-0.008	0.186
mother's occ class: 1	0.047	0.049	0.034	0.063	0.062	0.411
mother's occ class: 2	0.015	0.054	-0.112	0.074	1.352***	0.443
mother's occ class: 3	0.056	0.048	0.053	0.059	0.074	0.388
mother's occ class: 4	0.055	0.040	0.011	0.050	0.471	0.310
mother's occ class: 5	0.010	0.049	0.033	0.060	-0.199	0.420
mother's occ class: 6	0.025	0.040	0.029	0.046	0.021	0.313
mother's occ class: 7	0.055	0.041	0.058	0.049	-0.113	0.313
mother's occ class: 9	-0.004	0.038	0.037	0.046	-0.489*	0.287
mother's occ class: 10	0.004	0.032	-0.007	0.037	0.088	0.256
'nuclear family' to 16	0.028	0.019	-0.001	0.022	0.258*	0.135
mid 1990s	-0.045***	0.009	-0.050***	0.010	0.066	0.046
late 1990s	-0.065***	0.014	-0.070***	0.016	0.079	0.082
post 2000	-0.033	0.021	-0.040*	0.024	0.100	0.127
# observations	21256		21256		21256	
# individuals	2266		2266		2266	
R^2	0.265		0.042		0.242	

F-test on exclusion of smoking at 18 from first stage: 48.02; Partial R^2 of instrument = 0.0236

Notes: *** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level
Standard errors are clustered at the level of the individual and robust.
'nuclear family' to 16 means lived with both natural parents from birth to age 16.
Reference categories: West Midlands, white, did not live with both natutal parents to 16, father/mother occupational class 'plant/machine operative'. Occupational Class dummies: (1) management, (2) professional, (3) associate professional/technical, (4) clerical/secretarial, (5) craft and related, (6) personal/protective services, (7) sales, (9) other, (10) self-emp/unemp.

Table 3.10: Effect of Early Smoking on Probability of Being a Home-owner, Probit Model

	Home Owner		
	marginal fx	**z**	**x-bar**
log of hourly wage	0.204***	16.15	2.196
smoker at 16 indicator	-0.044***	-3.49	0.313
age	-0.004	-1.03	39.430
age^2	0.000***	2.93	1682.770
year-of-birth	0.022***	3.60	62.39 = 1958
year-of-birth^2	0.000***	-3.20	4025.480
region: North	-0.013	-0.42	0.069
region: Yorkshire	-0.067**	-2.22	0.096
region: North West	0.009	0.35	0.105
region: East Midlands	-0.043	-1.48	0.090
region: East Anglia	-0.038	-1.09	0.043
region: South East	-0.127***	-4.96	0.287
region: South West	-0.100***	-3.11	0.098
region: Wales	-0.075**	-2.00	0.050
region: Scotland	-0.107***	-3.17	0.075
ethnicity: black	-0.094	-1.57	0.006
ethnicity: asian	0.075**	2.25	0.017
ethnicity: other	0.045	0.72	0.007
father's occ class: 1	0.040*	1.93	0.134
father's occ class: 2	-0.004	-0.16	0.064
father's occ class: 3	0.021	0.65	0.038
father's occ class: 4	0.026	0.76	0.047
father's occ class: 5	0.015	0.82	0.222
father's occ class: 6	-0.028	-0.94	0.044
father's occ class: 7	-0.013	-0.35	0.032
father's occ class: 9	-0.087***	-3.08	0.080
father's occ class: 10	-0.002	-0.12	0.177
mother's occ class: 1	0.038	0.95	0.034
mother's occ class: 2	-0.021	-0.47	0.029
mother's occ class: 3	-0.046	-1.05	0.031
mother's occ class: 4	-0.007	-0.21	0.094
mother's occ class: 5	0.016	0.38	0.026
mother's occ class: 6	0.017	0.52	0.073
mother's occ class: 7	0.020	0.60	0.063
mother's occ class: 9	-0.017	-0.49	0.078
mother's occ class: 10	-0.034	-1.27	0.525
'nuclear family' to 16	0.017	1.21	0.825
mid 1990s	-0.004	-0.43	0.207
late 1990s	-0.024*	-1.66	0.224
post 2000	-0.057***	-2.86	0.385
observed prob.	0.829		
predicted prob. (at x-bar)	0.863		
# observations	24034		
# individuals	2615		

Notes: Reference categories: West Midlands, white, did not live with both natutal parents to 16, father/mother occupational class 'plant/machine operative'. Occupational Class dummies: (1) management, (2) professional, (3) associate professional/technical, (4) clerical/secretarial, (5) craft and related, (6) personal/protective services, (7) sales, (9) other, (10) self-emp/unemp.

Table 3.11: Effect of Early Smoking on Probability of Having Had a Dental or Optician Check-up in the Last Year, Probit Models

	Dental Check		Opticians Check		
	marginal fx	**z**	**marginal fx**	**z**	**x-bar**
log of hourly wage	0.132***	8.72	0.070***	5.92	2.196
smoker at 16 indicator	-0.040***	-2.67	-0.029***	-2.59	0.313
age	0.001	0.11	-0.007	-1.59	39.415
age^2	0.000	1.46	0.000***	3.55	1681.620
year-of-birth	0.039***	5.49	0.006	1.03	62.41 = 1958
year-of-birth^2	0.000***	-5.05	0.000	-0.88	4028.120
region: North	0.014	0.40	-0.004	-0.15	0.069
region: Yorkshire	-0.006	-0.20	-0.007	-0.30	0.096
region: North West	-0.034	-1.07	-0.015	-0.59	0.105
region: East Midlands	-0.039	-1.21	-0.012	-0.49	0.090
region: East Anglia	0.110***	2.79	-0.017	-0.57	0.043
region: South East	-0.048*	-1.81	-0.021	-1.02	0.287
region: South West	-0.003	-0.10	0.009	0.39	0.098
region: Wales	-0.058	-1.47	-0.008	-0.28	0.050
region: Scotland	-0.045	-1.25	0.005	0.20	0.075
ethnicity: black	0.001	0.01	-0.014	-0.19	0.006
ethnicity: asian	-0.151***	-2.71	0.051	1.13	0.017
ethnicity: other	-0.042	-0.52	0.128	1.62	0.007
father's occ class: 1	0.037	1.40	0.072***	3.24	0.135
father's occ class: 2	0.047	1.46	0.080***	2.87	0.064
father's occ class: 3	0.027	0.63	0.088**	2.57	0.038
father's occ class: 4	0.047	1.24	0.010	0.37	0.047
father's occ class: 5	0.013	0.54	0.022	1.16	0.222
father's occ class: 6	0.026	0.75	0.044	1.42	0.044
father's occ class: 7	0.058	1.30	0.040	1.07	0.032
father's occ class: 9	-0.011	-0.35	0.016	0.66	0.080
father's occ class: 10	0.026	1.02	0.022	1.01	0.178
mother's occ class: 1	0.049	1.03	-0.016	-0.42	0.034
mother's occ class: 2	0.059	1.15	-0.048	-1.10	0.029
mother's occ class: 3	0.031	0.59	0.018	0.43	0.031
mother's occ class: 4	0.061	1.63	-0.016	-0.49	0.094
mother's occ class: 5	-0.016	-0.31	-0.018	-0.40	0.026
mother's occ class: 6	0.008	0.22	-0.013	-0.41	0.072
mother's occ class: 7	0.105	2.68	0.049	1.38	0.063
mother's occ class: 9	-0.019	-0.49	-0.026	-0.85	0.078
mother's occ class: 10	0.024	0.74	-0.014	-0.53	0.526
'nuclear family' to 16	0.035***	1.94	0.019	1.27	0.825
mid 1990s	0.003	0.26	0.010	0.85	0.207
late 1990s	0.010	0.56	0.028*	1.69	0.224
post 2000	0.018	0.67	0.023	0.94	0.386
observed prob.	0.631		0.307		
predicted prob. (at x-bar)	0.636		0.302		
# observations	24086		24086		
# individuals	2615		2615		

Notes: Reference categories: West Midlands, white, did not live with both natutal parents to 16, father/mother occupational class 'plant/machine operative'. Occupational Class dummies: (1) management, (2) professional, (3) associate professional/technical, (4) clerical/secretarial, (5) craft and related, (6) personal/protective services, (7) sales, (9) other, (10) self-emp/unemp.

Table 3.12: Human Capital Earnings Function Estimations, OLS and IV using RoSLA

	OLS		IV: RoSLA		IV: first stage	
Dep. Var: log hourly wage	**Coeff.**	**Robust Std. Err.**	**Coeff.**	**Robust Std. Err.**	**Coeff.**	**Robust Std. Err.**
constant	-0.754***	0.250	-0.655**	0.280	-1.459	1.681
years of schooling	0.046***	0.003	0.102**	0.051	—	—
min. school LA=16	—	—	—	—	0.564***	0.206
age	0.099***	0.004	0.095***	0.005	0.056**	0.022
age^2	-0.001***	0.000	-0.001***	0.000	-0.001***	0.000
year-of-birth	-0.016**	0.007	-0.040*	0.023	0.427***	0.041
year-of-birth^2	0.000***	0.000	0.000**	0.000	-0.004***	0.000
region: North	0.047	0.038	0.051	0.041	-0.080	0.272
region: Yorkshire	0.003	0.033	-0.014	0.040	0.320	0.256
region: North West	0.054	0.032	0.033	0.041	0.386	0.255
region: East Midlands	-0.010	0.032	-0.007	0.035	-0.035	0.234
region: East Anglia	0.015	0.039	-0.001	0.047	0.324	0.327
region: South East	0.142***	0.028	0.101**	0.051	0.741	0.208
region: South West	0.023	0.034	0.017	0.038	0.114***	0.240
region: Wales	-0.012	0.040	-0.017	0.043	0.093	0.290
region: Scotland	0.028	0.036	-0.005	0.050	0.658**	0.266
ethnicity: black	0.114	0.105	0.114	0.110	0.037	0.746
ethnicity: asian	-0.136	0.071	-0.255*	0.139	2.146***	0.515
ethnicity: other	-0.048	0.103	-0.174	0.152	2.214**	1.074
father's occ class: 1	0.116***	0.028	0.051	0.069	1.162***	0.216
father's occ class: 2	0.121***	0.038	-0.013	0.128	2.404***	0.298
father's occ class: 3	0.089**	0.043	0.000	0.093	1.585***	0.333
father's occ class: 4	0.065*	0.036	-0.015	0.083	1.440***	0.308
father's occ class: 5	0.038*	0.023	0.020	0.029	0.322*	0.172
father's occ class: 6	0.014	0.035	-0.046	0.064	1.046***	0.313
father's occ class: 7	0.103***	0.040	0.078	0.049	0.484	0.339
father's occ class: 9	-0.021	0.029	0.012	0.044	-0.592***	0.196
father's occ class: 10	0.029	0.027	0.028	0.028	0.043	0.186
mother's occ class: 1	0.047	0.049	0.039	0.056	0.107	0.426
mother's occ class: 2	0.015	0.054	-0.065	0.094	1.378***	0.454
mother's occ class: 3	0.056	0.048	0.054	0.053	0.007	0.395
mother's occ class: 4	0.055	0.040	0.027	0.050	0.453	0.317
mother's occ class: 5	0.010	0.049	0.025	0.054	-0.240	0.430
mother's occ class: 6	0.025	0.040	0.027	0.042	-0.070	0.322
mother's occ class: 7	0.055	0.041	0.057	0.044	-0.053	0.324
mother's occ class: 9	-0.004	0.038	0.022	0.047	-0.491*	0.293
mother's occ class: 10	0.004	0.032	-0.003	0.034	0.103	0.264
'nuclear family' to 16	0.028	0.019	0.010	0.026	0.330**	0.137
mid 1990s	-0.045***	0.009	-0.048***	0.010	0.063	0.047
late 1990s	-0.065***	0.014	-0.068***	0.015	0.075	0.083
post 2000	-0.033	0.021	-0.038*	0.023	0.094	0.129
# observations	21256		21256		21256	
# individuals	2266		2266		2266	
R^2	0.265		0.177		0.227	
F-test on exclusion of min. school LA=16 from first stage: 7.49; Partial R^2 of the instrument = 0.0044						

Notes: *** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level
Standard errors are clustered at the level of the individual and robust.
'nuclear family' to 16 means lived with both natural parents from birth to age 16.
Reference categories: West Midlands, white, did not live with both natutal parents to 16, father/mother occupational class 'plant/machine operative'. Occupational Class dummies: (1) management, (2) professional, (3) associate professional/technical, (4) clerical/secretarial, (5) craft and related, (6) personal/protective services, (7) sales, (9) other, (10) self-emp/unemp.

Table 3.13: Human Capital Earnings Function Estimations, OLS and IV using RoSLA, Basic Specification

	OLS		**IV: RoSLA**		**IV: first stage**	
Dep. Var: log hourly wage	**Coeff.**	**Robust Std. Err.**	**Coeff.**	**Robust Std. Err.**	**Coeff.**	**Robust Std. Err.**
constant	-0.849***	0.247	-0.668**	0.303	-3.375*	1.727
years of schooling	0.052***	0.003	0.100**	0.042		
smoker at 16 indicator					0.691***	0.219
age	0.098***	0.004	0.095***	0.005	0.068***	0.023
age^2	-0.001***	0.000	-0.001***	0.000	-0.001***	0.000
year-of-birth	-0.014**	0.007	-0.038*	0.022	0.501***	0.042
year-of-birth^2	0.000***	0.000	0.000**	0.000	-0.004***	0.000
region: North	0.041	0.038	0.046	0.041	-0.089	0.286
region: Yorkshire	-0.003	0.033	-0.014	0.038	0.246	0.268
region: North West	0.050	0.033	0.030	0.041	0.418	0.273
region: East Midlands	-0.016	0.032	-0.009	0.035	-0.129	0.258
region: East Anglia	0.010	0.040	-0.001	0.045	0.277	0.343
region: South East	0.143***	0.028	0.099*	0.052	0.936***	0.223
region: South West	0.023	0.034	0.014	0.038	0.202	0.256
region: Wales	-0.018	0.040	-0.019	0.042	0.022	0.314
region: Scotland	0.020	0.036	-0.010	0.048	0.710**	0.288
ethnicity: black	0.117	0.093	0.108	0.105	0.251	0.700
ethnicity: asian	-0.150**	0.070	-0.248**	0.120	2.075***	0.560
ethnicity: other	-0.042	0.095	-0.167	0.149	2.566***	0.939
mid 1990s	-0.047***	0.009	-0.048***	0.010	0.036	0.050
late 1990s	-0.068***	0.014	-0.068***	0.015	0.013	0.089
post 2000	-0.038*	0.021	-0.037*	0.022	-0.004	0.137
# observations	21256		21256		21256	
# individuals	2266		2266		2266	
R^2	0.251		0.176		0.113	
F-test on exclusion of instrument from first stage: 9.98; Partial R^2 of the instrument = 0.0058						

Notes: *** significant at 1% level, ** significant at 5% level, * significant at 10% level.
Standard errors are clustered at the level of the individual and robust.

Table 3.14: Human Capital Earnings Function Estimations, OLS and IV using Smoker at 16 Status and RoSLA

	OLS		IV: both		IV: first stage	
Dep. Var: log hourly wage	**Coeff.**	**Robust Std. Err.**	**Coeff.**	**Robust Std. Err.**	**Coeff.**	**Robust Std. Err.**
constant	-0.754***	0.250	-0.613**	0.283	-0.157	1.663
years of schooling	0.046***	0.003	0.125***	0.019	—	—
smoker at 16 indicator	—	—	—	—	-0.874***	0.107
min. school LA=16	—	—	—	—	0.556***	0.202
age	0.099***	0.004	0.094***	0.005	0.054**	0.022
age^2	-0.001***	0.000	-0.001***	0.000	-0.001***	0.000
year-of-birth	-0.016**	0.007	-0.050***	0.011	0.399***	0.041
year-of-birth^2	0.000***	0.000	0.001***	0.000	-0.004***	0.000
region: North	0.047	0.038	0.053	0.044	-0.097	0.272
region: Yorkshire	0.003	0.033	-0.021	0.040	0.347	0.253
region: North West	0.054	0.032	0.024	0.039	0.409	0.253
region: East Midlands	-0.010	0.032	-0.006	0.037	-0.014	0.235
region: East Anglia	0.015	0.039	-0.008	0.048	0.398	0.325
region: South East	0.142***	0.028	0.084**	0.036	0.767***	0.207
region: South West	0.023	0.034	0.015	0.040	0.192	0.236
region: Wales	-0.012	0.040	-0.019	0.045	0.082	0.286
region: Scotland	0.028	0.036	-0.019	0.043	0.705***	0.263
ethnicity: black	0.114	0.105	0.115	0.116	-0.114	0.788
ethnicity: asian	-0.136	0.071	-0.305***	0.103	1.975***	0.493
ethnicity: other	-0.048	0.103	-0.226*	0.116	2.021*	1.080
father's occ class: 1	0.116***	0.028	0.024	0.040	1.118***	0.213
father's occ class: 2	0.121***	0.038	-0.068	0.062	2.271***	0.290
father's occ class: 3	0.089**	0.043	-0.038	0.056	1.485***	0.319
father's occ class: 4	0.065*	0.036	-0.048	0.050	1.324***	0.303
father's occ class: 5	0.038*	0.023	0.012	0.027	0.322*	0.170
father's occ class: 6	0.014	0.035	-0.070	0.046	0.968***	0.303
father's occ class: 7	0.103***	0.040	0.067	0.048	0.501	0.330
father's occ class: 9	-0.021	0.029	0.026	0.034	-0.542***	0.194
father's occ class: 10	0.029	0.027	0.027	0.030	0.000	0.185
mother's occ class: 1	0.047	0.049	0.036	0.060	0.079	0.414
mother's occ class: 2	0.015	0.054	-0.098	0.069	1.379***	0.442
mother's occ class: 3	0.056	0.048	0.053	0.057	0.018	0.388
mother's occ class: 4	0.055	0.040	0.016	0.048	0.451	0.310
mother's occ class: 5	0.010	0.049	0.030	0.057	-0.104	0.414
mother's occ class: 6	0.025	0.040	0.028	0.045	0.030	0.313
mother's occ class: 7	0.055	0.041	0.057	0.047	-0.111	0.316
mother's occ class: 9	-0.004	0.038	0.033	0.044	-0.488*	0.285
mother's occ class: 10	0.004	0.032	-0.005	0.036	0.099	0.256
'nuclear family' to 16	0.028	0.019	0.002	0.022	0.251*	0.136
mid 1990s	-0.045***	0.009	-0.049***	0.010	0.073	0.046
late 1990s	-0.065***	0.014	-0.070***	0.016	0.092	0.081
post 2000	-0.033	0.021	-0.039*	0.023	0.120	0.126
# observations	21256		21256		21256	
# individuals	2266		2266		2266	
R^2	0.265		0.088		0.250	

F-test on exclusion of instruments from first stage: 36.83; Partial R^2 of the instrument = 0.0332
Hansen's J-test of overidentification = 0.202, p-value = 0.6529

Notes: *** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level
Standard errors are clustered at the level of the individual and robust.
'nuclear family' to 16 means lived with both natural parents from birth to age 16.
Reference categories: West Midlands, white, did not live with both natutal parents to 16, father/mother occupational class 'plant/machine operative'. Occupational Class dummies: (1) management, (2) professional, (3) associate professional/technical, (4) clerical/secretarial, (5) craft and related, (6) personal/protective services, (7) sales, (9) other, (10) self-emp/unemp.

3.13 Figures

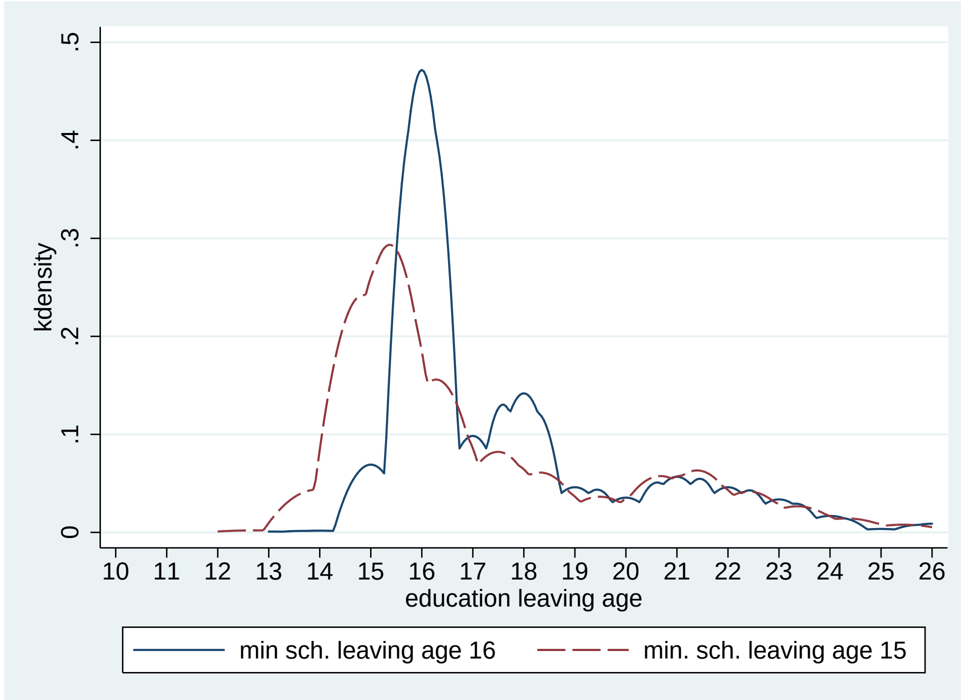


Figure 3.1: Education Leaving Age Density, by Minimum School Leaving Age

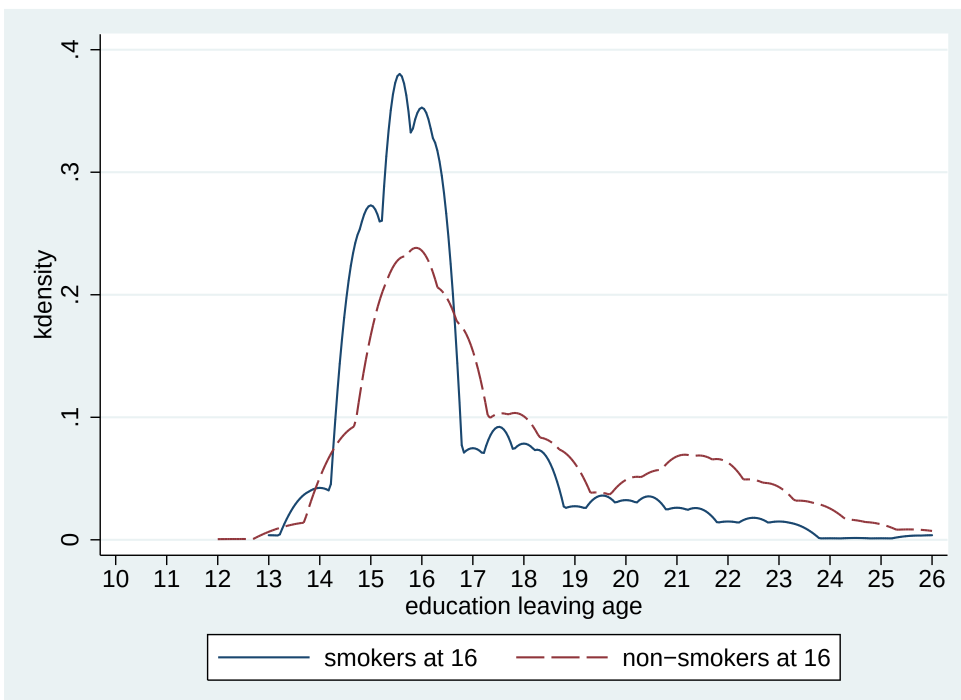


Figure 3.2: Education Leaving Age Density, by Smoker at 16 Status

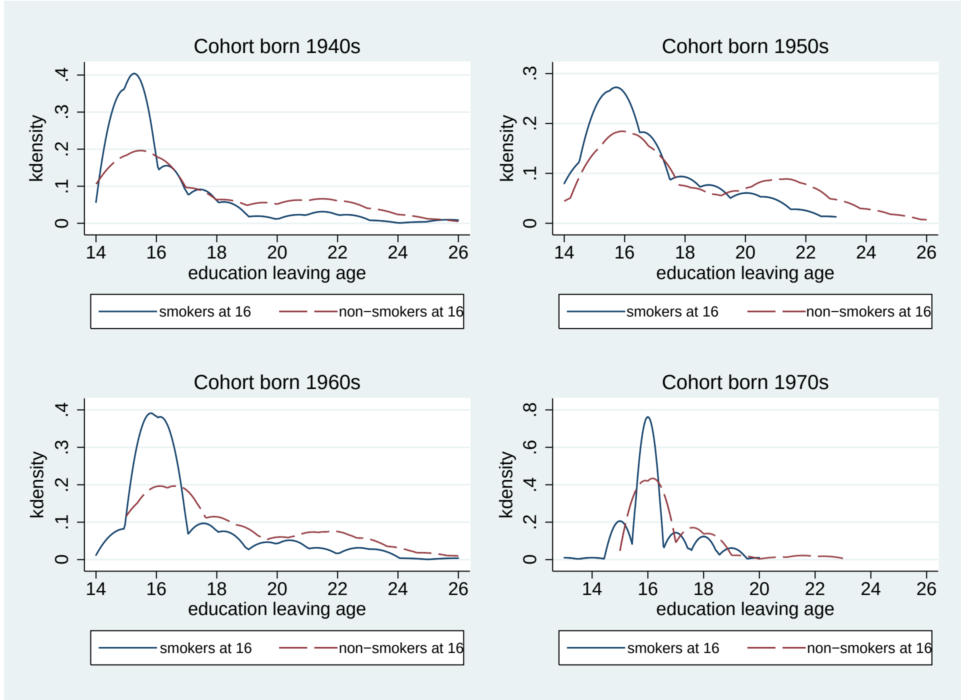


Figure 3.3: Education Leaving Age Density, by Smoker at 16 Status and Cohort

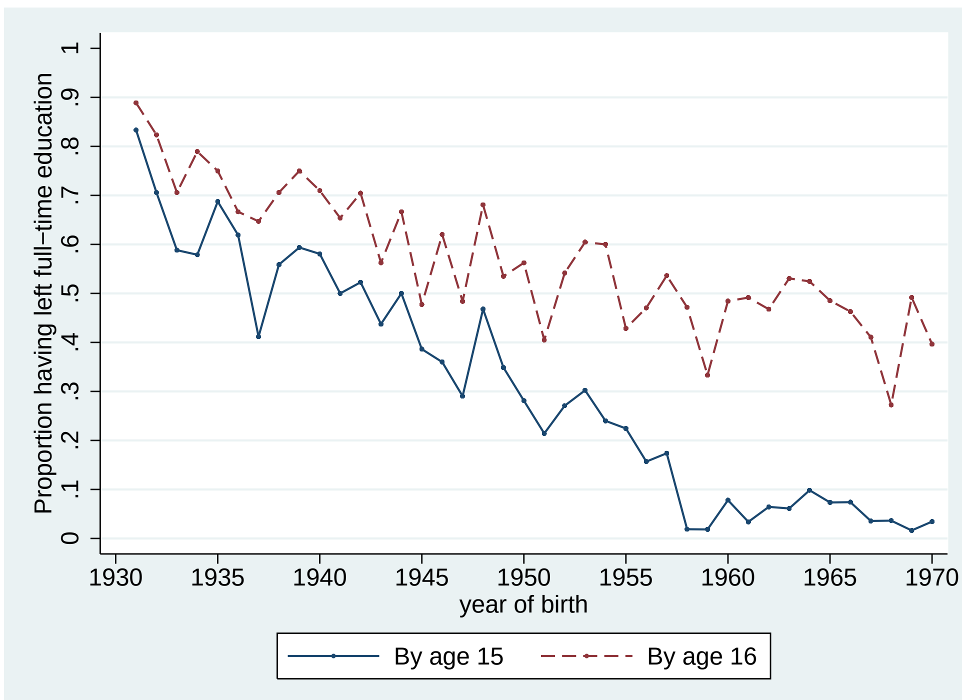


Figure 3.4: Proportion Left Full-Time Education at age 15 and at age 16

Chapter 4

The Lifetime Public Premium in Earnings: The View from Europe

4.1 Introduction

For a long time there has been debate both in society as whole and amongst labour economists, concerning whether or not public sector workers enjoy a "public premium" in earnings, compared to workers in the private sector. Understanding the differences in remuneration between the sectors has implications for recruitment, incentive structures and the human capital of the workforce in each sector, thus it is an important area of research. Studying the more recent literature, it seems that though the premium can on the face of it be very large for public sector workers, when differences between each sector's workforce in terms of observed and unobserved characteristics are taken into account, the premium either reduces substantially or disappears all together.

The vast majority of the literature in this area of public pay gaps concentrates on cross-sectional differences between the sectors in terms of earnings levels, and is concerned with analysing the extent to which non-random selection into sectors can explain the observed differences. However, the public and private sectors differ not only with regard to earnings level, but also in terms of earnings mobility and job mobility. Acknowledgement of this, and the fact that forward-looking individuals are likely to care about earnings mobility and job security in addition to earnings levels, motivated an earlier paper by Postel-Vinay and Turon (2007). In that paper, the authors contend that a more general assessment of the existence and magnitude of a "public premium" must be based not on comparisons of cross-sectional income flows but rather on measures of the lifetime value of employment in each sector. To that end, Postel-Vinay and Turon develop a dynamic model which simultaneously estimates individual income trajectories, employment dynamics and selection into sector, allowing for unobserved heterogeneity in individual patterns of earnings level and earnings mobility, as well as heterogeneity with regard to the propensity to become unemployed or to work in the public sector. The lifetime values of jobs in each sector are constructed and cross-sector comparative analysis of *these* values is undertaken.

As with the UK, we illustrate below that in many countries across Europe there are marked differences between the public and private sector with regard to earnings mobility, earnings volatility and job loss risk[1], as well as earnings levels. Therefore in assessing public-private pay gaps in Europe it is going to be equally important to base comparisons on a measure of the lifetime value of employment in each sector rather than just on instantaneous income flows. This is the first time that such an approach has been applied to European data.

We use the data from the European Community Household Panel over the period 1994-2001; specifically we apply a dynamic model akin to Postel-Vinay and Turon (2007) to data from Germany, the Netherlands, France, Italy, Spain and Portugal[2]. We were concerned to estimate the model within a common framework for each country, thus for each the specification of the dynamic model is the same, and we achieve a good fit in terms of job mobility, earnings distributions and earnings mobility in each of the six countries.

Our main findings can be summarised as follows. Firstly, after controlling for selection the average public premium in lifetime values (defined as the present discounted value of future earnings flows) would be positive in Germany, France, Spain and Portugal, but slightly below zero in Italy and the Netherlands, if an individual was planning to remain employed for their entire working life in either sector. Second, when allowing for the more realistic scenario of movement between labour market sectors, and controlling for selection, the average public premium becomes approximately zero for Germany, the Netherlands, Spain and Portugal. In Italy and France there is on average a small positive public premium in lifetime values. This suggests

[1]Using the same data that we use here (the ECHP), Clark and Postel-Vinay (2004) show that across Europe individuals perceive employment in the public sector to be more secure and less at risk in economic downturns than private sector jobs.

[2]More on why these countries and no others were used in section 4.3 below.

that in each country there is sufficient job mobility that individuals are quickly allocated to their 'natural' sector. Thirdly, given that the public premium in lifetime values, in most of the countries, remains uniform and close to zero as we move up the percentiles of the distribution, this suggests that where we do observe somewhat greater earnings compression in the public sector, it is due to a lower variance in the transitory component of earnings in the public sector.

The paper proceeds as follows: the related literature is reviewed in the next section, before a descriptive analysis of each country's data is presented in section 4.3. The statistical model to be estimated is detailed in section 4.4, with the results analysed in section 4.5. The lifetime values of employment in each sector are computed in section 4.6 allowing us to contrast the public-private differences accounting for earnings and job mobility with straightforward cross-sectional earnings differences. Section 4.7 concludes.

4.2 Related Literature

This paper relates to two different literatures: the public-private pay differences literature, and also the literature on income mobility and lifetime inequality. Moreover, within the public-private literature, this paper contributes by presenting an application of this dynamic modelling approach for a number of countries – whereas previously it has only been estimated for the UK – and also by deriving a set of estimates of public-private pay gaps across a number of major European countries, estimated with a common model on data from a common longitudinal data set.

As noted in the introduction, the majority of the public-pay gap literature concentrates on cross-sectional differences in wages and on the extent to which these can be explained by non-random selection into sector. In the UK for example, Disney and Gosling (2003) show that the raw public premium in male earnings is high, however when they use an instrumental variables approach – exploiting privatization to control for selection into the private sector – the premium becomes insignificant. For Germany, Dustmann and van Soest (1998) use a number of different specifications and consistently estimate that public sector wages are lower for all age and education groups but that this gap decreases with both age and education. They obtain very different results however if they do not take account of the endogeneity of sector choice or if the selection equation to take account of the endogeneity is only weakly identified. In addition they find that individuals who are observed in the public sector would have higher average earnings if they were to move to the private sector, however these workers' public dispremium is smaller than would be the case for those workers observed in the private sector – suggesting that workers do select into the sector where they have a comparative advantage. These results support the author's earlier findings for German males (Dustmann and van Soest (1997)) and are stronger given their robustness to model specification and controls for endogeneity.

More recently Melly (2005) has used quantile regression techniques and similarly found that conditional wages in Germany are lower in the public sector for males, and that the conditional distribution of wages is more compressed in the public sector – a finding that is common throughout the literature and across (most) countries. Melly suggests that the public sector effect on wages is not uniform across the wage distribution, with differences in characteristics explaining more of the public-pay gap at the top of the distribution, with differences in unobservables explaining more at the bottom. This supports the need to control for unobservable characteristics and their influence on sector selection and wages.

With respect to the Netherlands, Hartog and Oosterbeek (1993) deal with the endogeneity of sector choice by using an endogenous switching model to estimate public-private pay differentials. They refute earlier Dutch evidence of public sector underpayment, concluding that public sector workers earn more in the public sector than they would in the private sector while the reverse is true for private sector workers, indicating that workers sort into the sector affording them a comparative advantage. Similarly Van Ophem (1993) uses a modified endogeneous switching model and finds that while some categories of public sector workers earn more than corresponding workers in the private sector, there are several categories of employment – both higher and lower skilled – in which the public sector suffers a substantial dispremium. However, the results do indicate that the individuals who select into the public sector earn more than a randomly selected individual would earn in the public sector – therefore concurring with Hartog and Oosterbeek that in the Netherlands public sector workers have a comparative advantage in this sector.

Recently Bargain and Melly (2008) have used a large sample from the French Labour Force Survey – a rotating panel in which individuals are included for three successive years, with one third of the sample replaced each year – to estimate the public-private pay difference. They use both a standard fixed-effects estimator to control for selection and also implement a fixed-effects quantile regression model to evaluate the public effect at different quantiles of the distribution. Bargain and Melly find that in France on average men select negatively into the public sector but that the public (dis)premium becomes zero once selection on unobservables has been accounted for. The quantile regression results suggest that the often found result of pay compression in the public sector is partly due to positive selection of men into the public sector at the bottom of the distribution, but negative selection into the public sector at the top of the distribution. A positive premium being available for blue-collar workers, while the white-collars suffer a negative public premium.

Lassibille (1998) decomposes public-private wage differentials in Spain into the contribution of differences in worker characteristics and differences in the returns to those characteristics, using separately estimated wage functions that control for selection into sector. He finds that the public sector pays lower returns to education and experience, and thus the earnings advantage in the public sector is higher for the lower skilled but lowers as we move up the skill distribution. Lassibille also concludes that differences in worker characteristics are more important than differences in the returns to these characteristics in explaining the public pay gap, and that there is a public sector 'mark-up' in wages that is unrelated to characteristics, generally offsetting the lower returns on human capital.

Explicit cross-country comparison of public-private wage differentials is rare, however Lucifora and Meurs (2006) investigate public-pay gaps in Britain, France and Italy using non-parametric (kernel) and quantile regression methods. For France and Italy they conclude that the private sector use of collective bargaining and union power in these countries results in a pay setting system based heavily on rewarding observable characteristics (education, experience), which can explain the most part of the wage gap with the public sector. The quantile regression analysis echoes Melly's findings for Germany, suggesting that as we move up the distribution, the proportion of the pay gap explained by observable characteristics increases, such that in the lower quintiles differences in the unobserved characteristics of workers are more important in explaining pay differences. These results for France and Italy are substantiated by Ghinetti and Lucifora (2007) using ECHP data from the final wave, 2001.

Nevertheless, though these studies are informative and in some cases deal with the endogeneity of sector choice through either functional form assumptions (Van Ophem) or an instrumental variables approach (Dustmann and Van Soest, Hartog and Oosterbeek), they are still considering only cross-sectional differences in instantaneous earnings between the sectors. This is equally the case for those studies that employ a more sophisticated quantile regression approach considering the effects at different points in the distribution

Cappellari (2002) is the only other study (bar Postel-Vinay and Turon (2007)) to address differences in earnings dynamics between the public and private sector. He uses Italian administrative data, creating a panel that allows him to control for unobserved individual heterogeneity in earnings levels and earnings growth rates. In the analysis, Cappellari considers cross-sector differences in the autocovariance structure of log wages, finding that there is lower earnings dispersion in the public sector yet greater persistence: thus while inequality is lower in the public sector, initial earnings differences persist over the lifecycle. However, while considering the importance of differences in earnings mobility and volatility as well as levels, Cappellari deals with the public and private sectors separately, thus imposing the assumption of exogenous selection of individuals into sectors and taking no explicit account of transitions between sectors or into unemployment. As many studies attest to the critical importance of non-random sorting of workers across sectors, we model the employment dynamics alongside the earnings dynamics in order to form a more complete picture.

This paper also relates to the vast literature on empirical models of income dynamics and their application to the study of lifetime income inequality. Within this broad literature there are a number of approaches, though the majority of contributions (including ours) use flexible reduced form models of either absolute or relative earnings mobility to decompose the earnings process into a permanent and a transitory component. Differences between individuals in the permanent component are interpreted as a measure of lifetime inequality (see *inter alia* Lillard and Willis, 1978; Gottschalk and Moffit, 1993; Gottschalk, 1997; Bunchinsky and Hunt, 1999; Bonhomme and Robin, 2004). A second line of attack is to take a more structural approach derived from job search theory and analyze inequality in lifetime values and how this inequality has evolved over time (see Bowlus and Robin, 2004; Flinn, 2002). While each of these papers contributes to the body of evidence on lifetime inequality in earnings, none of them consider lifetime differences between job sectors.

So while there is a large literature considering the public-private pay gap in a *cross-sectional* context, controlling for the non-random nature of sector choice, and a large literature considering lifetime inequality in earnings using dynamic models, the marriage of the two is very rare, a gap that this paper fills for Europe[3].

4.3 Descriptive Data Analysis

4.3.1 *The European Community Household Panel*

We use data from the European Community Household Panel (henceforth ECHP) which is a longitudinal survey of households and individuals carried out in 15 European Union countries annually between 1994 and 2001.[4] The ECHP was centrally designed and co-ordinated by the Statistical Office of the European Communities (Eurostat), and aimed to be representative of the population of the EU, living in private households, at both the individual and household level. The objective of the survey was to be both cross-sectionally and longitudinally representative, each country's sample evolving as new members join sample households through births or new relationships and new households are formed by original survey members leaving one household to start another. A great advantage of the ECHP design is the standardisation of the survey across countries and over time, creating a consistent set of variables covering a wide range of topics, and allowing comparisons to be made both across countries and over time.

Unfortunately, the cost of this comparability comes in the breadth of the survey within each country, with prohibitively small sample sizes in some countries (see below).

Within each country, we restrict our sample to males in order to avoid issues around female labour market participation, such as part-time work, maternity rights and home production. We also drop from the sample anyone who is retired or otherwise economically inactive. Moreover, we exclude individuals who have never entered the labour market i.e. young men who are yet to leave full-time education. Among those who are working we restrict the sample to full-time workers (full-time defined as working 30+ hours per week) and we only include the observations for individuals aged from 20 to 60 in their first observation. We define three 'sectors' of labour

[3]The 'gap' in the literature for the UK having been addressed by the earlier Postel-Vinay and Turon (2007) paper.

[4]Initially only the 12 countries that were members of the EU in 1994 were included: Belgium, France, Italy, Luxembourg, the Netherlands, Germany, Denmark, Ireland, the UK, Greece, Portugal and Spain. In addition, Austria, Finland and Sweden joined in waves 2, 3 and 4 respectively.

market activity: employment in the private sector, employment in the public sector, and unemployment.[5] As we believe that individuals care about their total monthly earnings – which depends on both their hourly wage and their monthly hours – we use current gross monthly earnings,[6] reported once per year and deflated using each country's CPI. Within each country the data is also detrended to remove common real income trends. We trim the earnings data by treating earnings observations below the 2nd and above the 98th percentile of earnings within each 'education'x'job sector' cell as missing data.[7]

The rules governing inclusion in the sample, added to the relatively small population size of some of the countries involved in the ECHP, results in sample sizes that are too small to implement our model in Belgium, Luxembourg, Denmark, Greece, Austria, Finland and Sweden. However, we do retain a usable sample in six countries: Germany, the Netherlands, France, Italy, Spain and Portugal[8].

4.3.2 *Germany*

Basic Sample Description

For Germany the constructed sample retains 3026 men, who each have between 4 and 8 consecutive observations, 6.8 being the average.[9] Of the 3026 men, 2208 (73.0%) are initially observed in private sector employment, and remain in the sample for an average of 6.8 years. 612 individuals (20.2%) are initially observed in the public sector and are retained in the sample for 6.9 years on average. The remaining 206 men (6.8%) are initially unemployed and remain in the sample for an average of 6.3 years.

The ECHP includes a standardised education measure – the ISCED classification[10] – coded into 3 categories: "high" is ISCED levels 5-7 and corresponds to all classes of tertiary level education, "medium" is ISCED level 3, corresponding to upper-secondary (post-compulsory)

[5] In addition to those reporting themselves to be unemployed, the unemployment category includes: working unpaid in a family enterprise, in education or training (though having been in the labour market at some point), and doing housework, looking after children or other persons.

[6] The ECHP variable pi211mg, which is in the national currency of each country.

[7] We do not drop these *observations* only replace their earnings as missing. Therefore the individuals concerned still convey information to the sample and contribute to the modelling of the labour market dynamics.

[8] Results using the UK sample in the ECHP, which is itself taken from the BHPS, concur with those found by Postel-Vinay and Turon (2007) when using a larger sample available in the British Household Panel Survey.

[9] There is some sample attrition, 14.4% after 4 years and 47.3% at 8 years, which we assume to be exogenous. Some of the attrition is a consequence of our sample construction rules that treat individuals as censored from the first time they have a gap in their response history (this affects 6.5% of the individuals in our sample).

[10] International Standard Classification of Education.

education, and "low" is ISCED levels 0-2, corresponding to levels of education up to the end of secondary schooling. In terms of education, the German sample breaks down overall as 27.2% high, 61.4% medium, and 11.3% low[11]. This masks some differences between the job sectors, the breakdown for the private sector being 25.4% high, 62.0% medium and 12.6% low, as compared with the public sector: 35.0% high, 59.8% medium and just 5.2% low. The public sector therefore attracting markedly more high educated and fewer low educated individuals. In terms of age and labour market experience, the public sector workers are on average a little older than those in the private sector (41.7 years old versus 39.4) and on average have slightly more experience[12] (21.9 years versus 20.6).

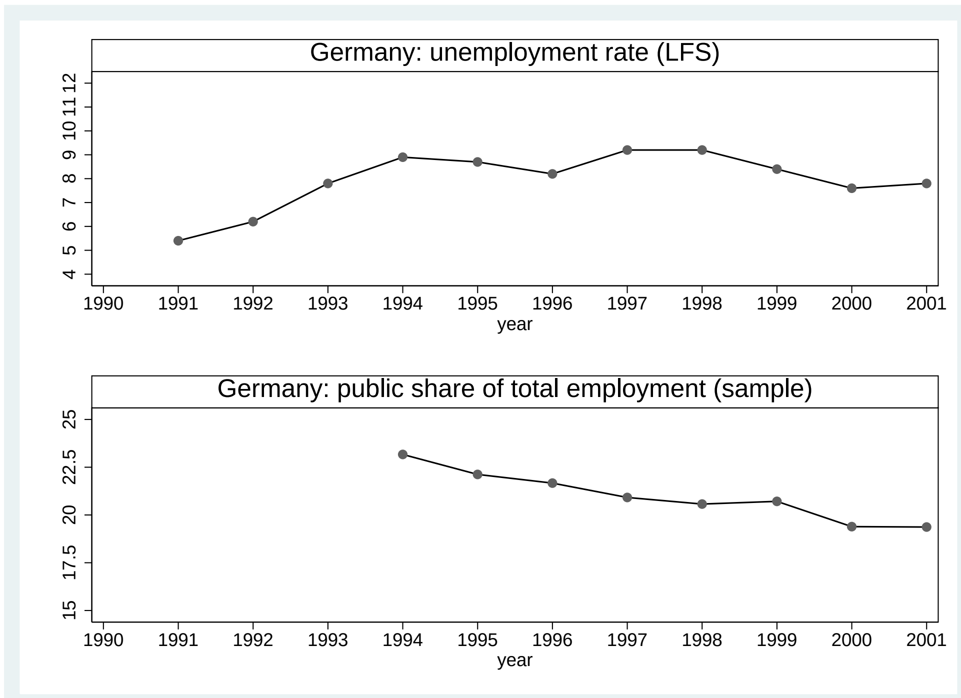


Figure 4.1: **Germany**: Unemployment rate, males, 1991-2001, and Sample public sector share

To illustrate the evolution of the German labour market over time, Figure 4.1 shows the male unemployment rate[13] for the years 1991-2001 (top panel), thus covering the years preceding our sample as well as the sample years themselves, and the public sector share of total employment for our sample (bottom panel). Both are reasonably stable over the time of our sample, the unemployment rate rising in the early nineties before levelling out at around 9% from 1994

[11] See appendix C.1 for a tabulation of the education breakdown for each country.

[12] 'Labour market experience' or more accurately '*potential* labour market experience' is defined as current age minus the age when the individual first entered the labour market.

[13] For each country the unemployment rate is calculated using data from the Labour Force Survey.

onwards. The public sector share of total employment falls slowly from around 23% at the start of the sample to just under 20% by 2001.

Differences in Earnings

Earnings levels. We now illustrate public-private differences in earnings through a number of simple regressions, see Table 4.1. For each country we will be looking at (log) current gross monthly earnings. Differences in monthly work hours for full-time workers could lead us to understate any positive public premium in hourly wages and differences in cross sectional wage variances. However, in Germany the public and private sectors have very similar hours distributions – median weekly work hours is 40 for each – though with the public sector exhibiting less variance in hours (standard deviation of weekly hours is smaller by around one hour and a quarter).

The first column of Table 4.1 shows that the raw public pay gap in our sample is 5.2 log points (around 5.3%) in favour of the public sector. However this positive premium appears to be driven to a large extent by selection. When controlling for a quadratic in potential labour market experience and education band (column 2) the public premium falls to 1.4 log points and is statistically insignificant. Moreover, allowing the effects of education and experience to differ between the sectors (column 3), results in a statistically significant negative public premium of 18.8 log points (17.1%). This negative public premium is reduced for the medium educated compared with the high or low educated and also is reduced in experience. Allowing for fixed individual effects[14] in a specification conditioning on just the quadratic in experience (column 4) reduces the size of the negative public premium and it is not significant. However introducing the interactions of the public sector dummy with education and the quadratic in experience in the fixed effects model (column 5) finds the public premium to be -13.2 log points (-12.4%) and significant. This is in line with the findings of Dustmann and van Soest (1998) who consistently find a significant negative public premium, reducing in age (experience), and robust to various modelling assumptions. Believing the final (column 5) fixed effects specification, we conclude that the returns to experience are greater in the public sector, such that the public premium traces an inverted U-shape in experience with a maximum estimated at 25 years. For levels of

[14] The reported fixed-effect regressions use the within-estimator. First differenced estimates are very similar for Germany.

experience between 16 and 34 years there is in fact a small positive public premium.

Earnings dispersion. The standard deviation of log earnings in the public sector (0.324) is smaller than in the private sector (0.358), while the 90:10 percentile ratio of raw earnings are 2.323 and 2.522 respectively. Conditional on age and education these ratios are 2.129 (public) and 2.394 (private). These figures indicate a greater degree of wage compression in the public sector.

Earnings mobility. The regression results and the analysis of the earnings distributions show that the public and private sectors differ in terms of their earnings levels and the cross-sectional distribution of earnings. Moreover, there are differences in terms of earnings mobility, as illustrated in the upper panel of Table 4.2 which shows the transition matrices between quintiles of the unconditional log earnings distribution from one year to the next for the public and private sectors respectively.[15] The matrices show that there is greater persistence in earnings rank amongst those individuals continuously employed in the public sector than amongst those continuously employed in the private sector. To further describe the persistence in earnings levels, the lower panel of the Table contains the corresponding transition matrices for the rank of earnings residuals after conditioning on education and a quadratic in (potential) labour market experience. Again we see the greater level of persistence of earnings rank in the public sector.

Bearing in mind that the earnings distribution is more spread in the private than the public sector, transitions between earnings quintiles in the private sector represent larger changes in earnings than similar transitions in the public sector, further underscoring the greater mobility of earnings in the private sector. In addition, we computed the 1-lag auto-covariance of normalised log earnings for each individual, having conditioned on education and a quadratic in (potential) labour market experience.[16] For individuals employed in the private sector in consecutive periods the mean auto-covariance is 0.811 whereas the corresponding figure for the public sector it is higher at 0.944, again illustrating the greater persistence in earnings in the public sector.

[15] The transitions relate to individuals who are employed in the same sector in year *t-1* and year *t*. It is possible to look at longer lags in our sample (up to 7 lags) however the numbers of individuals who are continuously employed in either sector at longer lags is not sufficiently large to allow robust inference.

[16] We constructed normalised log earnings by first regressing log earnings (y_{it} for individual i at date t) on the covariates from the column 2 specification in Table 4.1, thus obtaining a predictor of mean earnings $\widehat{y}_{it}$. We then regressed the squared residuals from this latter regression on that same set of covariates to obtain a predictor of earnings variance $\widehat{\sigma}_{it}^2$. We then constructed earnings disturbances as $(y_{it} - \widehat{y}_{it})/\widehat{\sigma}_{it}$. Following this we calculated the one-period auto-covariance between these disturbances, and compare the average for those in the public sector for both periods with those in the private sector for both periods.

Differences in Job Mobility

The transition matrix below shows the changes in employment sector between one wave and the next, with rows referring to sector in year *t-1*, columns to sector in year *t*:

	Private	Public	Unemp.
Private	94.1	1.2	4.5
Public	6.6	91.1	2.1
Unemp.	36.8	5.2	57.8

It is clear that movement directly from the private sector to the public sector is very uncommon: only 1.2% of individuals initially employed in the private sector move to the public sector the next year; however movements in the opposite direction are more frequent (6.6%). The annual transition rate into unemployment from the private sector (4.5%) is more than double the corresponding figure for the public sector (2.1%), suggesting more job losses from the private sector.[17] This greater security of employment in the public sector is also reflected in the differing probabilities of ever being observed in unemployment depending on observed initial sector. Of those first observed employed in the private sector 20.5% are recorded as unemployed at least once in their following observations, compared with only 7.8% for those initially employed in the public sector.

Of those unemployed, 57.8% remain unemployed in the next year, 36.8% gaining employment in the private sector, while just 5.2% of those unemployed in year *t-1* are in public sector employment in year *t*. Of the 707 individuals (23.4% of the sample) ever observed unemployed during the time-span of the sample, 23.9% report being unemployed for 3 or more consecutive interviews during the sample. The 1-year re-employment probability for these "long-term" unemployed is considerably worse at 13.5% than it is for the "short-term" unemployed who have a 70.4% chance of finding a job in the following year. Thus unemployment persistence is quite high overall and there is some evidence that it is concentrated on the "long-term unemployed".

As well as differential re-employment rates according to whether the individual is a "long-term unemployed" type, there is also evidence of attachment to the public sector: an unemployed individual who has most recently been employed in the public sector is much more likely to

[17] These are not pure job loss rates as voluntary quits are included in movements from employment to unemployment.

find employment in the public sector in the next year than an individual who's most recent employment was in the private sector. Of the unemployed who were most recently in the public sector 8.3% return to public sector employment the next year, whereas for those unemployed who were most recently in the private sector, only 4.3% find public sector employment in the next year.

4.3.3 *The Netherlands*

Basic Sample Description

The sample constructed for the Netherlands includes 2213 men, each with between 4 and 8 observations, with the average being 6.6.[18] Of these 2213 men, 1638 (74.0%) are first observed in the private sector and remain in the sample for an average of 6.6 years. A further 519 individuals (23.5%) are initially observed in the public sector and are retained in the sample for 6.7 years on average. The remaining 56 men (2.5%) are first encountered in unemployment and remain in the sample for an average of 5.6 years.

With regard to education, the Netherlands sample breaks down overall as 24.8% "high", 55.1% "medium" and 20.0%"low". There are however, considerable differences between the sectors in terms of education composition: the private sector breaking down as 20.9% high, 57.2% medium and 21.9% low, whereas the corresponding public sector figures are respectively 41.5%, 45.2% and 13.3%. The public sector thus attracts substantially more better educated workers and fewer medium and low educated workers than the private sector. In terms of age and labour market experience, the public sector employees are on average more than three-and-a-half years older (43.4 years old versus 39.7) and have more (potential) experience (23.6 years compared to 20.7 years).

Figure 4.2 illustrates the evolution of the Netherlands labour market over time, the top panel plotting the male unemployment rate for the years 1990-2001 (thus encapsulating our sample time frame plus the preceding 4 years), while the lower panel shows the public sector share of total employment in our sample. The unemployment rate is steady at around 5-6% prior to the start of the sample period in 1994, but then falls gradually over the following years to be around

[18]There is some sample attrition, 16.0% after 4 years and 53.3% at 8 years, which we assume exogenous. Some of the attrition is a result of our sample selection rules that censor individuals from the first gap in their response history (this affects 7.2% of the individuals in our sample.)

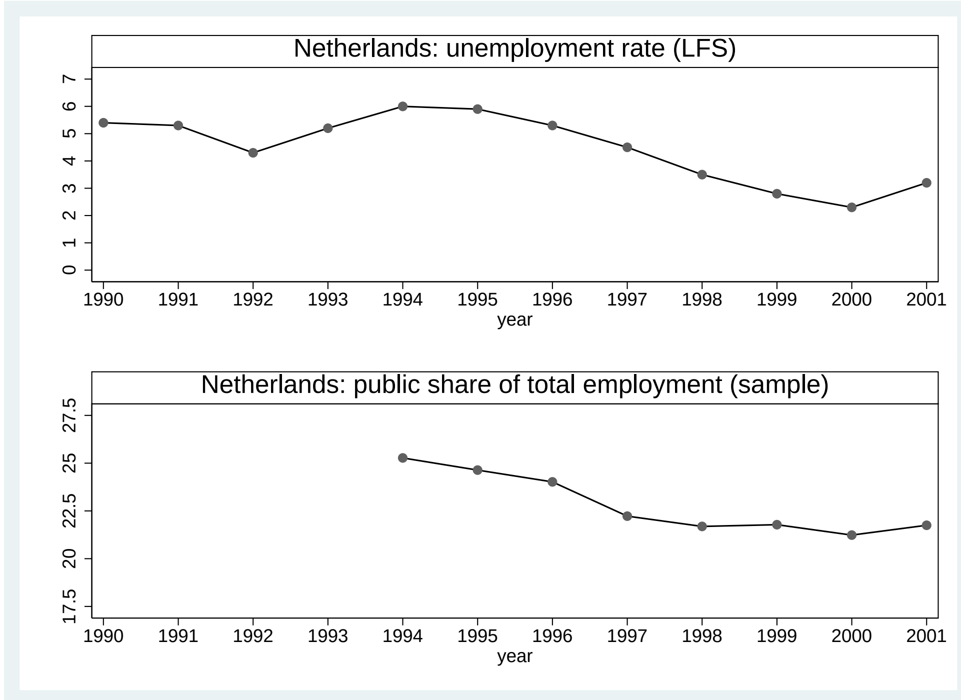


Figure 4.2: **Netherlands**: Unemployment rate, males, 1991-2001, and Sample public sector share

3% in 2001. The public sector share of total employment remains largely stable in the sample, though falling steadily from 25% in 1994 to just under 22% in 2001.

Differences in Earnings

Earnings levels. Table 4.3 describes public-private differences in earnings levels through a number of simple regressions, where again the dependent variable is the log of current gross monthly earnings. The hours distributions for full-time workers are similar for each sector, though median weekly hours is slightly less in the public sector (38 versus 40); therefore by looking at monthly earnings we will slightly under-estimate any positive public premium in hourly wages. As we might expect, the distribution of hours in the public sector is less spread than in the private sector, the standard deviation of weekly hours is smaller by approximately one hour and a half.

The first column of Table 4.3 shows that the raw public pay gap in the Netherlands sample is 9.4 log points (9.8%) in favour of the public sector. However, selection effects are clearly important as when we control for a quadratic in potential labour market experience and education band (column 2) the public pay premium is estimated to be −3.3 log points (−3.2%) and significant. Moreover, when we allow the effects of education and experience to differ between the sectors (column 3) the public premium is estimated to be -9.7 log points (-9.3%) and remains significant. This is the public premium for "high" education workers, and the premium for "low" education workers is not significantly different, however, for "medium" band educated men the premium is estimated to be almost exactly zero. The public premium does not however vary with experience. Estimating the model including individual-level fixed effects[19] and controls for a quadratic in potential labour market experience (column 4), the public premium is estimated to be -0.2 log points and is not statistically significant. Introducing interactions between the public sector dummy and education and the quadratic in experience results in a positive estimate of the public premium of 4.5 logs points (4.6%) though the estimate is imprecise (t=1.46) According to this latter (column 5) fixed effects specification, returns to experience are higher in the private sector for all levels of experience, and as such the small positive public premium is eroded with experience, the premium tracing a U-shape in experience with an estimated minimum at 36 years. The public premium is negative for all levels of experience greater than 12 years.

Earnings dispersion. The standard deviation of log earnings in the public sector (0.298) is smaller than in the private sector (0.322) and the slightly greater compression of pay in the public sector is also reflected in the 90:10 percentile ratios of raw earnings which are 2.098 and 2.317 respectively. The corresponding ratios conditional on age and education are 1.800 (public) and 1.962 (private), further illustrating the greater degree of pay compression in the public sector.

Earnings mobility. The sections above illustrate that the public and private sectors differ in terms of the cross-sectional distribution of earnings. In addition, there are differences in terms of earnings mobility, as illustrated in the upper panel of Table 4.4 which shows the transition matrices between quintiles of the unconditional log earnings distribution from one year to the next for the public and private sectors respectively.[20] These matrices show that there is greater persistence in earnings rank amongst those individuals continuously employed in the public sector than amongst those continuously employed in the private sector. Moreover, the lower panel of the Table, which shows the transitions for log earnings residual rank after conditioning

[19]The reported fixed effects results are estimated using the within estimator. First differenced estimates are similar for the Netherlands.

[20]See footnote 15.

on education and a quadratic in (potential) labour market experience, illustrate the same pattern of greater earnings persistence in the public sector.

Since the earnings distribution is slightly more compressed in the public sector than in the private sector, transitions between earnings quintiles in the private sector represent larger changes in earnings than similar transitions in the public sector, further underscoring the greater mobility of earnings in the private sector. In addition, we computed the 1-lag auto-covariance of normalised log earnings for each individual, having conditioned on education and a quadratic in (potential) labour market experience. For individuals employed in the private sector in consecutive periods the mean auto-covariance is 0.874 whereas for the public sector this figure is higher at 0.884, again illustrating the greater persistence in earnings in the public sector.

Differences in Job Mobility

The following transition matrix shows the changes in employment sector between one wave and the next, with rows referring to sector in year *t-1*, columns to sector in year *t*:

	Private	Public	Unemp.
Private	96.5	1.9	1.4
Public	7.8	90.8	1.3
Unemp.	40.3	6.8	52.7

Few individuals (1.9%) initially employed in the private sector move directly to employment in the public sector from one year to the next, and though movements in the opposite direction are more common, still only 7.8% of individuals employed in the public sector in year *t-1* move into the private sector in year *t*. The average annual transition rate into unemployment is slightly greater in the private sector, 1.4%, compared with 1.3% in the public sector. This similarity in unemployment risk is also reflected in the different probabilities of ever being observed in unemployment depending on the sector in which the individual is first observed. For men initially observed in the private sector, the probability of ever experiencing unemployment in the sample is 7.4%, which is only slightly above the corresponding probability for those first observed in the public sector, 6.6%. Just over half (52.7%) of the unemployed remain so in the following period, with 40.3% exiting to private sector employment, the other 6.8% gaining employment in the public sector. Of the 212 individuals (9.6% of the sample) who ever experience unemployment during the sample, 20.8% report being unemployed for 3 or more consecutive periods during the time-span of the sample. These "long-term" unemployed have only an 8.5% probability of finding a job in the next period, whereas for the "short-term" unemployed the probability is 84.5%. Thus though aggregate unemployment persistence is not that high, with just under half of the unemployed in any year finding employment by the next interview, the persistence appears to be highly concentrated on the "long-term" unemployed.

There is evidence of attachment to the public sector in the different rates of entry to public sector employment from unemployment, depending on whether the unemployed individual's last employment was in the public sector. Of the unemployed whose most recent employment was in the public sector, 19.5% re-entered public sector employment by the time of the next interview. For those unemployed whose most recent employment was in the private sector, the public sector entry probability is just 4.2%.[21]

4.3.4 *France*

Basic Sample Description

For France the sample constructed retains 2281 men, each with between 4 and 8 observations, 6.2 observations on average.[22] Of the 2281 individuals, 1566 (68.7%) are initially observed in private sector employment and remain in the sample for an average of 6.2 years. A further 547 (24.0%) individuals are first observed employed in the public sector and remain in the sample for 6.3 years on average. The remaining 168 men (7.4%) are first observed unemployed and they remain in the sample for an average of 5.6 years.

In terms of education, the France sample breaks down overall as 25.8% "high", 45.7% "medium" and 28.5% "low". There is a difference between the sectors in terms of education composition: the private sector having 23.8% high educated workers, 46.5% medium and 29.7% low, with the corresponding public breakdown 33.3%, 43.5%, 23.2%. So the proportions of medium educated workers are very similar between the sectors, with the main difference being that the public sector attracts more high educated and fewer low educated workers. The public

[21] These public sector entry rates have to be treated with care as very few people are observed unemployed in any single year and of these around one fifth have not been observed in employment before. As a result the public sector entry rates for those most recently observed in the public sector derive from only 41 individuals.

[22] There is some sample attrition, 26.0% after 4 years and 60.7% at 8 years, we assume this to be exogenous. Some of the attrition is the result of our sample construction rules which treat an individual as censored from the first gap in their response history (this affects 21.4% of our sample).

sector workers are also on average a couple of years older than those in the private sector (41.7 years old versus 39.5) and have a couple more years of potential labour market experience (22.6 years versus 20.8).

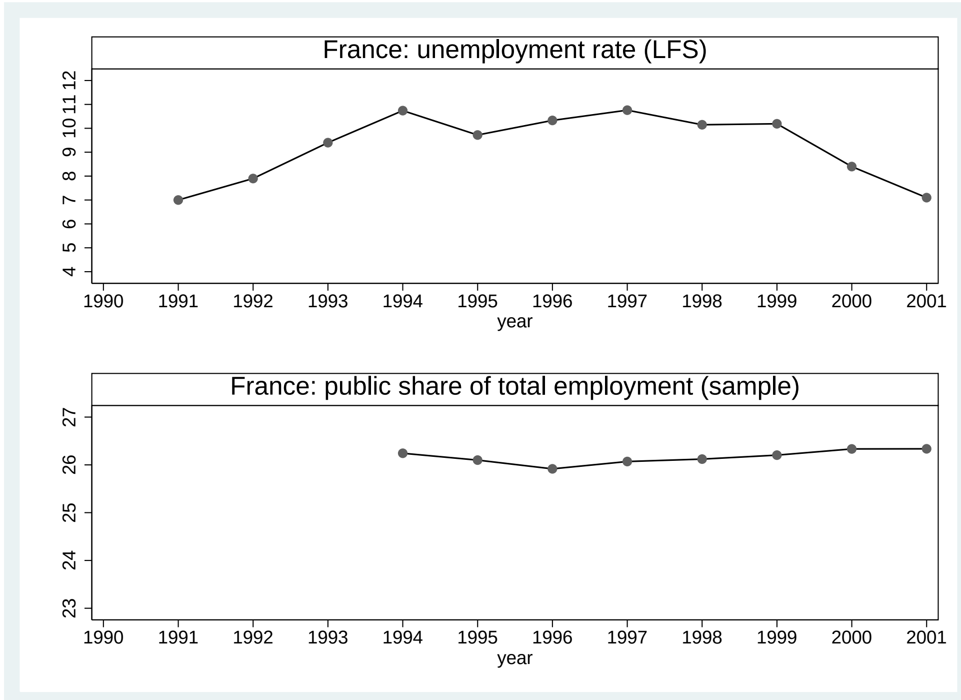


Figure 4.3: **France**: Unemployment rate, males, 1991-2001, and Sample public sector share

To illustrate the pattern of the French labour market over time, Figure 4.3 plots the unemployment rate for the years 1991-2001 (top panel) thus covering our sample period plus the years leading up to it, and the public share of total employment in our sample (bottom panel). The unemployment rate was rising steadily from 7% in 1991 to almost 11% by 1994 and the start of our sample. During the sample period the unemployment rate remains largely stable around 10% before slightly dropping down towards the end of the millennium. The public share of total employment however remains almost totally constant through the sample time frame, 26% to the nearest percent in each year.

Differences in Earnings

Earnings levels. The public-private differences in earnings levels are now described through the simple regressions in Table 4.5 in which the dependent variable is the log of current gross monthly earnings. The hours distributions for full-time workers are similar for each sector (both have median weekly hours of 39) though as we might expect, the public sector hours distribution has less variation, considerably less in fact: the standard deviation of weekly hours is smaller by just over two hours.

Column 1 of Table 4.5 shows that the raw public pay gap in the French sample is 12.5 log points (13.3%) in favour of the public sector. Some of this positive public premium is the result of selection however, as conditioning on education band and a quadratic in (potential) labour market experience (column 2), reduces the premium to 4.1 log points (4.2%) though it remains strongly significant. This significant positive premium appears to be driven by those in the "low" education band, as when we allow for different public premia depending on education and experience (column 3), the coefficient on the public sector dummy becomes insignificantly different to zero, while the interaction term for low educated public sector workers is strongly significant, the coefficient of 0.131 suggesting a 13.9% public premium for these workers. This concurs with the findings of Bargain and Melly (2008) who only find a positive public premium for blue collar workers. Allowing for fixed individual effects[23] and conditioning on just the quadratic in experience (column 4), the estimated public premium is no longer significant. Introducing the interaction between the public dummy and the quadratic in experience (column 5), the public premium is estimated to be 3.8 log points but has a large standard error (t=0.55). The interaction between public and experience-squared is significant and suggests that the public premium is initially falling for the first decade or so of work before rising as the individual acquires more labour market experience. This fixed effects specification (column 5) suggests that returns to experience are lower in the public sector for the first half of a working lifetime such that the small positive public premium initially falls with experience (minimum of zero premium estimated at 13 years experience) before rising again as public sector returns to experience increase above private returns in the second half of a working lifetime.[24]

Earnings dispersion. The standard deviation of log earnings is lower in the public sector than in the private sector (0.396 versus 0.430) illustrating the greater wage compression in the public sector. This is supported by the 90:10 ratios of raw earnings which are 2.730 (public) and 3.068 (private) with corresponding figures conditional on age and education of 2.404 and 2.688

[23]The reported fixed-effects regressions use the within-estimator. First differenced estiamtes are very similar for France.

[24]It should be borne in mind that in the fixed effects regressions, the basic public premium is identified from only 58 individuals (2.62% of the sample) who are observed in both sectors during the life of the sample.

respectively.

Earnings mobility. Looking at the regressions and the dispersion of earnings, we see that the public and private sectors differ in terms of their cross-sectional earnings distributions. Moreover, the sectors differ with respect to earnings mobility, as illustrated in the top panel of Table 4.6 which shows the transition matrices between unconditional raw earnings quintiles from one year to the next for the public and private sectors respectively.[25] The matrices show that there is greater persistence in the lower earnings quintiles rank amongst those individuals continuously employed in the public sector than amongst those continuously employed in the private sector, whereas in the higher earnings quintiles the level of persistence is very similar in each sector. This pattern is also exhibited in the transition matrices for ranks of log earnings conditional on education and a quadratic in (potential) labour market experience (lower panel of the Table).

As the earnings distribution is slightly more compressed in the public sector than in the private sector, transitions between earnings quintiles in the public sector do not represent quite so large changes in earnings as similar transitions in the public sector, thus the fact that private and public sector mobility is similar would indicate larger rises (falls) in earnings in the private sector. The 1-lag auto-covariance of normalised log earnings, having conditioned on education and a quadratic in (potential) labour market experience, is markedly greater for individuals employed in the public sector for consecutive periods (mean of 0.950) than it is for individuals in the private sector for consecutive periods (mean of 0.828). This also suggests greater persistence in earnings in the public sector.

Differences in Job Mobility

The transition matrix below shows the changes in employment sector between one wave and the next, with rows referring to sector in year *t-1*, columns to sector in year *t*:

	Private	Public	Unemp.
Private	96.9	0.3	2.7
Public	0.8	98.2	0.8
Unemp.	33.4	4.6	61.8

[25] See footnote 15.

It is immediately clear that movements directly from one employment sector to the other from one year to the next are exceedingly rare: only 0.3% of individuals employed in the private sector in year *t-1* are employed in the public sector in year *t*, with movements in the opposite direction relatively more frequent but still extremely rare (0.8%). The average annual rate of transition into unemployment from the private sector while low at 2.7% is still more than three times the corresponding job loss rate for the public sector (0.8%), suggesting that job security is greater in the public sector. This greater job security of the public sector is also shown in the probability of ever being observed unemployed if an individual is initially observed in public sector employment, which is only 4.2%. The corresponding figure for the private sector is 10.8%.

Unemployment persistence is quite high, 61.8% of those who are unemployed in year *t-1* remain unemployed in year *t*. Only 4.6% of the unemployed will exit to public sector employment by the next interview, whereas 33.4% exit to employment in the private sector. This aggregate rate of entering public sector employment after being unemployed disguises a difference between those whose most recent employment prior to becoming unemployed was in the public sector and those whose most recent employment was in the private sector. The public employment rate of the unemployed who have most recently been employed in the public sector is 9.1% whereas for those who were in the private sector before becoming unemployed, the chance of gaining public sector employment is only 3.3%, indicating that there is a certain degree of public sector attachment.[26]

Of the 360 individuals (15.8% of the sample) who are ever observed unemployed during the time-span of the sample, 27.5% report being unemployed in three or more consecutive interviews. The 1-year re-employment rate for these "long-term" unemployed is considerably lower, 15.6%, than it is for "short-term" unemployed who have a 67.5% chance of gaining employment by the next interview.

[26]The figure for public sector re-employment has to be treated with caution as it relates to only 44 individuals who have been public employed most recently before becoming unemployed.

4.3.5 *Italy*

Basic Sample Description

The Italy sample retains 2820 men, each with between 4 and 8 observations, the average being 6.4.[27] Of these 2820 individuals, 1639 (58.1%) are initially observed in private sector employment and remain in the sample for an average of 6.4 years. There are 786 men (27.9%) who are first observed in the public sector and remain in the sample for 6.6 years on average. The other 395 individuals (14.0%) are initially unemployed and remain in the sample for an average of 5.9 years.

In terms of education, the Italy sample breaks down overall as 9.0% "high" education level, 51.6% "medium" and 39.4% "low". The overall break down masks some substantial differences between the two sectors: the private sector comprises 6.6% high, 50.0% medium and 43.4% low educated workers, compared with 14.3%, 56.6% and 29.2% respectively for high, medium and low educated workers in the public sector. Thus the public sector attracts better educated workers, with more than double the proportion of high educated workers compared with the private sector, a greater proportion of medium educated also, with a much smaller proportion of low educated workers. Public sector workers are on average markedly older than private sector workers (43.3 years old compared to 37.8) and have more potential labour market experience (average of 24.5 years compared to 20.2).

Figure 4.4 illustrates the evolution of the Italian labour market over time, the top panel showing the unemployment rate for our sample period and the 4 years preceding it, the bottom panel showing the public sector share of total employment in our sample. The unemployment rate is largely stable at around 7.5% for the years preceding our sample period (which begins in 1994) before rising to just over 8.5% in 1994 and remaining around this level throughout the majority of the sample before declining back down towards 7.5% in the final years of the sample. The public share of total employment in the sample remains almost constant, declining slightly over the sample period from around 34% in 1994 to 32% in 2001.

[27] There is some sample attrition, 19.2% after 4 years and 60.7% at 8 years, which we assume to be exogenous. Some of the attrition is a consequence of our sample construction rules which treat an individual as censored from the first time that they have a gap in their response history (this affects 11.3% of our sample).

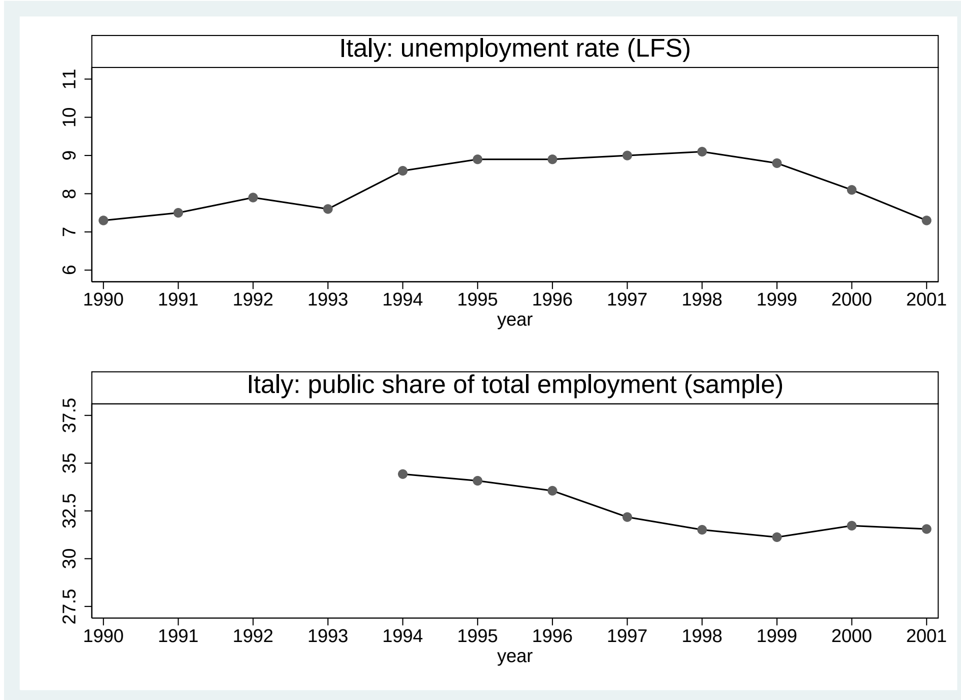


Figure 4.4: **Italy**: Unemployment rate, males, 1991-2001, and Sample public sector share

Differences in Earnings

Earnings levels. Table 4.7 describes public-private differences in earnings levels through a number of simple regressions, where again the dependent variable is the log of current gross monthly earnings. There is a definite difference between the sectors in terms of their hours distribution for full-time workers, with the public sector working fewer hours on average: median 36 hours per week compared with 40 in the private sector; therefore we will tend to underestimate any positive public premium in hourly wage. There is also less of a spread in hours in the public sector, the standard deviation in weekly hours is lower by around fifty-five minutes.

The first column of Table 4.7 shows that the estimated raw public pay gap in the data is 8.6 log points (9.0%) in favour of the public sector. However, conditioning on education band and a quadratic in potential labour market experience (column 2) the estimated premium is not significantly different to zero. This changes when we allow the effect of the public sector to vary according to education band and experience (column 3), the public premium is then estimated to be statistically significant and 21.0 log points (23.4%), though falling with experience. This specification suggests that the premium does not vary depending on the education band. Estimating the model with individual-level fixed effects controlling for a quadratic in experience (column 4) the public premimum is not significantly different from zero.[28] Introducing interactions of the public sector dummy with the quadratic in experience (column 5), the public premium is still not statistically significant but is estimated to be around 5.4 log points (5.5%), with a large standard error resulting in a t-statistic of only 1.15.[29] This latter fixed effects specification (column 5) suggests that returns to experience are greater in the private sector at all levels of experience, such that the small public premium traces a U-shape in experience, becoming negative at 12 years of experience and remaining negative thereafter, with an estimated minimum of -4.4 log points at 34 years of experience.

Earnings dispersion. In each sector the standard deviation of log earnings is almost identical, 0.293 in the private sector as compared with 0.296 in the public sector. The 90:10 percentile ratios in raw earnings suggest however that the private sector distribution is more spread, 2.058 (private) against 1.944 (public), with corresponding figures conditional on age and education being 1.897 (private) and 1.844 (public) respectively. These figures indicate that the extent of wage compression is similar in each sector though still greater in the public sector.

Earnings mobility. The analysis of earnings level and dispersion suggest that in Italy the public and private sectors differ in terms of their earnings levels while the overall compression of earnings is similar for each. Moreover, the sectors differ with respect to earnings mobility, as illustrated in the top panel of Table 4.8 which shows the transition matrices between the quintiles of the unconditional raw earnings distributions from one year to the next for the public and private sectors respectively.[30] In these matrices we can see that there is greater persistence in rank in all of the earnings quintiles for those individuals continuously employed in the public sector compared with those continuously employed in the private sector, though in the higher earnings quintiles the level of persistence is very similar. This pattern is also exhibited in the transition matrices for ranks of log earnings conditional on education and a quadratic in (potential) labour market experience (lower panel of the Table) and contributes to a picture of generally greater earnings persistence in the public sector.

Given that the earnings distributions are very similarly spread in each sector, the greater

[28]The reported fixed effects regressions use the within-estimator. First differenced estimates for column 4 produces very similar results.

[29]Estimating column 5 using first-differences produces similar results though with the public premium 13.3 log points and significant.

[30]See footnote 15.

frequency of transitions between earnings quintiles in the private sector suggests greater earnings mobility in the private sector in terms of the actual earnings received. Moreover, computing the 1-lag auto-covariance of normalised log earnings for each individual, having conditioned on education and a quadratic in (potential) labour market experience, we find greater auto-covariance of earnings for individuals employed in the public sector for successive periods. The mean auto-covariance for these individuals is 0.882, compared to 0.736 for individuals employed in the private sector for successive periods.

Differences in Job Mobility

The transition matrix below shows the changes in employment sector between one wave and the next, with rows referring to sector in year *t-1*, columns to sector in year *t*:

	Private	Public	Unemp.
Private	95.2	1.9	2.7
Public	4.8	94.0	1.0
Unemp.	21.9	4.7	73.3

Movement directly from private sector employment in year *t-1* to public sector employment in year *t* is very rare: only 1.9% of individuals initially in private sector employment are observed the following year employed in the public sector, though movements in the opposite direction are more frequent (4.8%). The annual rates of transition into unemployment are small for each sector, though the private sector rate (2.7%) is more than double the public (1.0%) suggesting greater job security in the public sector. This greater security of public sector employment is also reflected in the probability of ever being observed out of employment depending on the sector the individual is first observed in: of those initially observed in the public sector, only 3.7% are ever observed unemployed during the course of the sample, whereas for those first observed in the private sector, 11.5% are subsequently observed unemployed.

Of the unemployed in year *t-1*, 73.3% remain unemployed in year *t*. 21.9% gain employment in the private sector, while just 4.7% move into public sector employment. Of the 612 individuals (21.7% of the sample) ever observed unemployed during their sample observations, 51.3% record being unemployed for three or more of their consecutive interviews. This "long-term" unemployed status has a great effect on the annual re-employment probability, reducing it to 12.8% compared with a 69.2% 1-year re-employment rate for the "short-term" unemployed. Thus unemployment persistence overall is high, with almost three-quarters of the unemployed remaining so in the next interview, and there is some evidence that this is particularly the case for the "long-term" unemployed.

In addition to differing re-employment probabilities according to "long-term" unemployment status, there is some evidence of public sector attachment: an unemployed individual whose most recent employment was in the public sector has a 15.8% chance of gaining a public sector job in the next period, compared with only a 3.3% chance for those unemployed whose most recently observed employment was in the private sector. The overall re-employment rate for those whose most recent employment was in the private sector is however slightly higher (33.6%) than it is for those most recently employed in the public sector (28.6%).

4.3.6 *Spain*

Basic Sample Description

For Spain the sample comprises 2622 men, each with between 4 and 8 observations, with an average of 6.3 observations.[31] Of the 2622 individuals, 1646 (62.8%) are initially observed in private sector employment and remain in the sample for an average of 6.4 years. There are 524 men (20.0%) originally observed in the public sector, and they are each in the sample for 6.5 years on average. The remaining 452 individuals (17.2%) are initially unemployed, and they are retained in the sample for an average of 5.9 years.

In terms of education, the Spain sample breakdown overall is 33.6% "high", 23.0% "medium" and 43.4% "low". However this masks vast differences in educational composition between the sectors. For the private sector the breakdown is 28.7%, 23.2% and 48.1% for high, medium and low respectively, whereas for the public sector it is 51.0%, 21.1% and 27.9%. Thus the public sector contains a much greater proportion of high educated workers and a much lower proportion of low educated workers than the private sector. The proportion in the medium education band is very close for each sector, all of the difference being in the top and bottom of the respective distributions. Additionally, the public sector workers are on average older than those in the

[31] There is some sample attrition, 20.5% after 4 years and 58.9% at 8 years, which we assume to be random. Some of the attrition is a result of our sample construction rules which treat an individual as censored from the first gap in their response history (this affects 10.0% of our sample).

private sector (41.8 years old versus 38.6) and have more (potential) labour market experience (23.0 years versus 21.6).

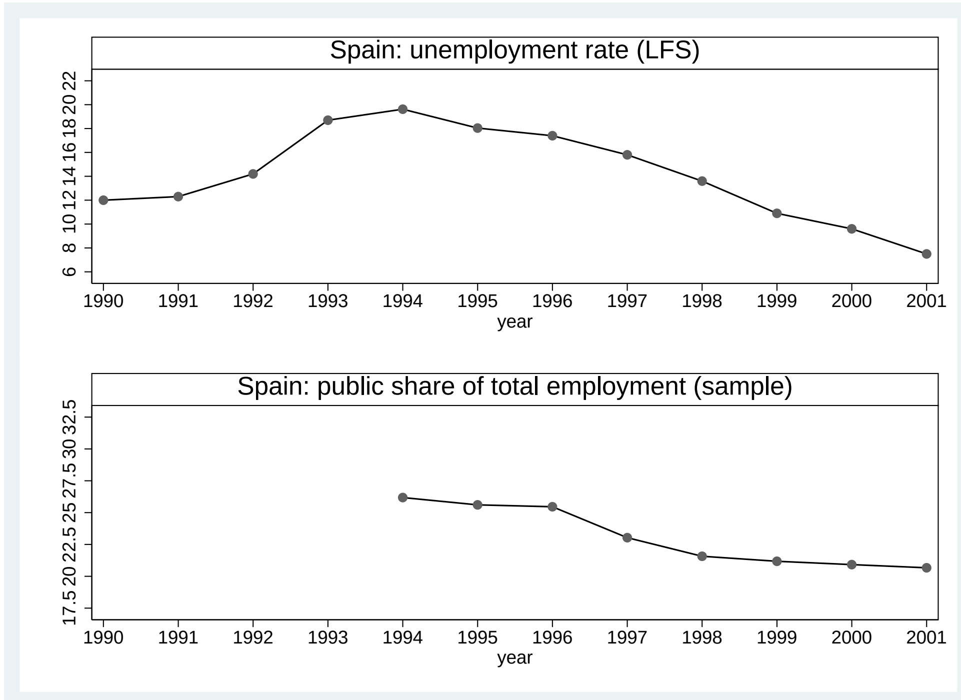


Figure 4.5: **Spain**: Unemployment rate, males, 1991-2001, and Sample public sector share

Figure 4.5 illustrates the evolution of the Spanish labour market over time, showing the unemployment rate for the years 1990-2001 (top panel), thus covering the years immediately preceding our sample as well as the sample years themselves, and the public sector share of total employment in our sample (bottom panel). We can see from the figure that in the years leading up to the start of the sample in 1994, the unemployment rate in Spain was rising quite sharply from around 12% in 1990 to almost 20% in 1994. During the course of the sample however, unemployment falls steadily and is down to 7.5% by 2001. The public sector share of total employment is stable at around 25-26% for the first three years of our sample before dropping slightly in 1997 (to 23%) and then continuing to fall such that is was just under 21% in 2001.

Differences in Earnings

Earnings levels. We now illustrate the public-private differences in earnings through a number of simple regressions, see Table 4.9. Again, in each case the dependent variable is the log of current gross monthly earnings. Between the sectors there are some differences in the monthly work hours distributions for full-time workers. Though the median weekly work hours are the same for each sector (40 hours), in the private sector there are very few workers who work below the median hours and a large spike at the median with a few smaller concentrations of workers working more hours than this. For the public sector there is a smaller mass at the median and more mass at points below median hours, with much fewer working more than the median hours. Therefore we will under-estimate any positive public premium in hourly wages. Moreover, the public sector exhibits much less variance in hours (standard deviation of weekly hours is smaller by almost one hour and a half).

Column 1 of Table 4.9 shows that the raw public pay gap in our sample is very large, 26.7 log points (30.6%), in favour of the public sector. This is unsurprising given the great differences in educational, and to a lesser extent age, composition of the respective workforces. Controlling for education and a quadratic in experience (column 2), the premium is reduced to 9.7 log points (10.2%). Allowing the public effect to differ according to education band and experience (column 3), we find no different effect of public sector employment dependent on education but the premium does vary with experience, from 27.0 log points (30.9%) prior to any labour market experience, to a low of 8.5 log points (8.9%) at 28 years experience. Estimating with individual-level fixed effects and controlling for just a quadratic in experience (column 4), the public premium is not significantly different to zero.[32] However, when we add to this fixed effects specification the interaction terms between the public dummy and experience and its square, the public premium is significant and estimated to be 16.2 log points (17.6%). Believing this latter fixed effects specification, we conclude that the returns to experience are consistently lower in the public sector, the initially large public premium tracing a U-shape in experience, turning negative after 25 years of experience and remaining negative for almost all the rest of a working lifetime. The estimated minimum of the public premium is -1.7 log points (-1.7%) and comes at 37 years of experience.

Earnings dispersion. There is a greater degree of wage compression in the public sector, illustrated in the standard deviation of log earnings, which is 0.395 in the public sector compared to 0.427 in the private sector. The 90:10 percentile ratios in raw earnings, 2.803 and 2.965 for the public and private sectors respectively, support this conclusion, as do the corresponding ratios

[32]The reported fixed-effects regressions use the within-estimator. The first differenced estimate is very similar for Spain specification 4.

controlling for age and education: 2.354 for the public sector and 2.706 for the private sector.

Earnings mobility. The regression results and analysis of the earnings distributions show that there are differences between the public and private sectors in terms of earnings levels and cross-sectional distributions. Furthermore, the extent of earnings mobility differs between the two sectors, as illustrated by Table 4.10 which shows the transition matrices between unconditional log earnings quintiles from one year to the next for the public and private sectors respectively.[33] These matrices show that persistence in earnings rank is greater amongst individuals continuously employed in the public sector compared to those continuously employed in the private sector; and this is especially true at the lower end of each distribution. Moreover, the lower panel of the Table, which shows the transitions for log earnings residual rank after conditioning on education and a quadratic in (potential) labour market experience, illustrate the same pattern of greater earnings persistence in the public sector.

Since the earnings distribution is less compressed in the private than in the public sector, transitions between quintiles in the private sector represent greater rises (falls) in earnings than similar transitions in the public sector, which further emphasizes the difference in earnings mobility between the sectors. Comparing the average one-lag auto-covariance of normalised log income, after controlling for education and a quadratic in (potential) labour market experience, the greater auto-covariance for individuals employed for consecutive periods in the public sector (0.890 versus 0.751, the corresponding figure for the private sector) adds to the picture of more persistent earnings in the public sector.

Differences in Job Mobility

The transition matrix below shows the changes in employment sector between one wave and the next, with rows referring to sector in year *t-1*, columns to sector in year *t*:

	Private	Public	Unemp.
Private	91.8	1.6	6.5
Public	7.4	89.9	2.5
Unemp.	39.2	5.4	55.3

[33] See footnote 15.

From the table it is apparent that movements directly from private sector employment to public sector employment in the following year is rare, only 1.6% of those in the private sector in year *t-1* are employed in the public sector in year t, however movements from public-to-private happen with greater frequency (7.4%). The annual transition rate into unemployment is, for the public sector, less than half what it is for the private sector (2.5% public versus 6.5% private) suggesting greater job security in the public sector. This is further underscored by the differing probabilities of ever being observed in unemployment depending on the sector in which an individual is first observed. Of those first observed in the private sector, 25.6% are ever subsequently observed unemployed, while for individuals first observed in the public sector, the figure is only 8.2%.

Of the unemployed in year *t-1*, 55.3% remain unemployed the following year, with 39.2% exiting to private sector employment, and the other 5.4% finding employment in year t in the public sector. There are 917 individuals (35.0% of the sample) who are ever observed unemployed during the time-span of the sample. Of these, 28.8% report being unemployed in three or more consecutive interviews during the course of their observations. These "long-term" unemployed have an 18.9% chance of finding employment in the next period, while for the "short-term" unemployed the average annual re-employment rate is much higher at 73.0%. Thus though unemployment persistence is quite low overall, it appears to be concentrated on the "long-term" unemployed.

There is also evidence of public sector attachment, in the differing public sector re-employment probabilities between the unemployed whose most recent observed employment was in the public sector and those whose most recent employment was in the private sector. For the former group, the probability of gaining a public sector job by the next year is 17.2% while for the latter group it is only 3.4%.

4.3.7 *Portugal*

Basic Sample Description

The constructed sample for Portugal comprises 2242 men, each with between 4 and 8 consecutive observations, with an average of 6.6 observations.[34] Of these 2242 individuals, 1638 (73.1%) are first observed in private sector employment and remain in the sample for 6.5 years on average. A further 447 (19.9%) are initially observed in the public sector and are retained in the sample for an average of 6.8 years. The remaining 157 individuals (7.0%) are first observed unemployed and remain in the sample for 6.1 years on average.

With respect to education, the Portugal sample overall comprises 7.8% "high" educated, 15.6% "medium" educated and 76.6% "low" educated individuals. This overall picture conceals some marked differences between the public and private sectors: the breakdown for the public (resp. private) sector is 15.6% high, 20.0% medium, 64.4% low (6.0% high, 14.4% medium, 79.6% low). Thus the public sector attracts markedly higher educated workers with a substantially larger proportion of high educated workers and a greater proportion of medium educated workers. Moreover public sector workers are on average more than 4 years older than private sector workers (41.5 years old versus 37.3) and have more than 3 years more potential labour market experience on average (23.7 years versus 20.4).

To illustrate the evolution of the Portuguese labour market over time, Figure 4.6 shows the unemployment rate from 1990-2001 (top panel), covering our sample period and the 4 years prior to it, and the public share of total employment for our sample (bottom panel). In the years prior to the start of the sample (in 1994), the unemployment rate rises gently from just above 3% in 1990 to just below 6% in 1994. During the sample period it is initially stable before falling relatively sharply between 1997 and 1998 and then continues to fall more gently to be just above 3% once again at the end of the sample period (2001). The public share of total employment in our sample falls over the sample window, from just under 25% in 1994 to just above 20% in 2001, with a large part of this fall being in 1998. Portugal was undergoing a period of privatisations through the mid-1990s and this is reflected in the fall in the public share of

[34] There is some sample attrition, 18.4% after 4 years, 52.32% at 8 years, which we assume to be exogenous. Some of the sample attrition is a result of our sample construction rules which treat an individual as censored from the first time that they have a gap in their response history (this affects 5.3% of our sample).

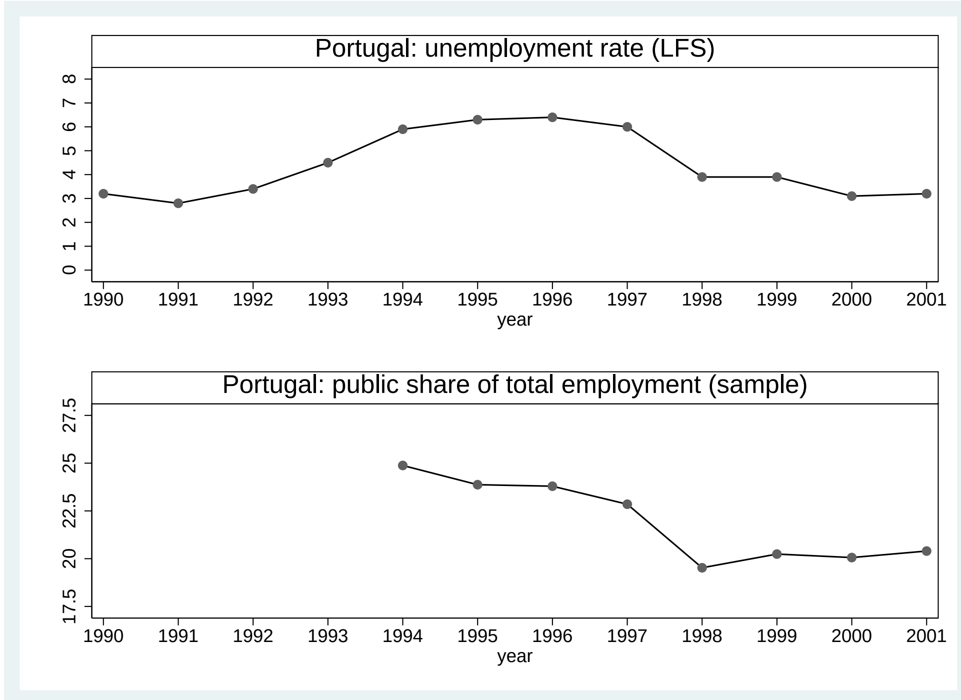


Figure 4.6: **Portugal**: Unemployment rate, males, 1991-2001, and Sample public sector share

employment in the mid-to-late-1990s.

Differences in Earnings

Earnings levels. Public-private differences in earnings are now illustrated through a number of simple regressions, see Table 4.11. Again, in each case the dependent variable is the log of current gross monthly earnings. There are marked differences between the sectors in terms of hours worked by full-time workers, despite each having a median of 40 hours work per week. In the private sector there are very few workers who work below the median hours and a large spike at the median, and another smaller spike above the median at around 45 hours per week. For the public sector there is a smaller mass at the median and more mass below it, with another spike at 35 hours. Therefore we will tend to under-estimate any positive public premium in hourly wages. In terms of variance both sectors are very similar, the standard deviation of weekly hours is larger by about 12 minutes in the public sector.

The first column of Table 4.11 shows that the raw public pay gap in out sample is 29.5 log points (34.3%) in favour of the public sector. However, it appears that this is driven to a large extent by selection: controlling for education band and a quadratic in (potential) experience (column 2), the positive public premium is reduced to 10.9 log points (11.5%) but is still statistically significant. Allowing the effect of public sector employment to vary with education band and experience (column 3) suggests that the premium does not vary with education but is affected by experience, the premium before any labour market experience being significant and *negative* (-17.2 log points) but increasing with experience. Allowing for individual fixed-effects and controlling for a quadratic in labour market experience (column 4), the public premium is estimated to be almost zero and not statistically significant.[35] Augmenting this specification to allow interactions between the public dummy and the quadratic in experience (column 5) we again estimate the public premium to be almost zero and not statistically significant.

Earnings dispersion. Unlike in the other countries that we consider, there is much greater earnings dispersion in the *public* sector than in the private sector in Portugal. The standard deviation of log earnings is 0.392 in the private sector, while in the public sector it is 0.511. The greater spread of earnings in the public sector is also reflected in the 90:10 percentile ratios of raw earnings, which are 2.563 for the private sector but 3.806 for the public sector. The corresponding ratios conditional on age and education are 2.281 and 2.626 respectively for the private and public sectors. Thus there is more earnings dispersion in the public sector than in the private sector, even after controlling for age and education.

Earnings mobility. The regressions and analysis of the earnings distributions show that there are differences between the public and private sectors in terms of earnings levels and cross-sectional distributions. Moreover, the extent of earnings mobility differs between the two sectors, as illustrated by Table 4.12 (top panel) which shows the transition matrices between unconditional log earnings quintiles from one year to the next for the public and private sectors respectively.[36] The matrices show persistence in earnings rank to be much greater amongst individuals continuously employed in the public sector compared to those continuously employed in the private sector. This is confirmed in the lower panel of the Table which shows the transition matrices for ranks of the residual of log earnings conditional on education and a quadratic in (potential) labour market experience. Persistence is consistently greater for those continuously employed in the public sector.

Though the private sector earnings distribution is more compressed than that of the public

[35]The reported fixed-effects regressions use the within estimator. Estimates from first differences provide very similar results.

[36]See footnote 15.

sector, these relatively large differences in rank transitions suggest that the private sector exhibits a greater degree of earnings mobility than the public sector. So the public sector has a larger range of earnings but individual's earnings within that range are more persistent, whereas in the private sector the range of earnings is somewhat smaller but there is more movement of individual earnings within this range. Furthermore, computing the one-lag auto-covariance of normalised log income, after controlling for education and a quadratic in (potential) labour market experience, we find greater auto-covariance for those employed for consecutive periods in the public than those employed for consecutive periods in the private sector (0.929 versus 0.816).

Differences in Job Mobility

The transition matrix below shows the changes in employment sector between one wave and the next, with rows referring to sector in year *t-1*, columns to sector in year *t*:

	Private	Public	Unemp.
Private	94.8	2.5	2.6
Public	10.6	88.4	0.9
Unemp.	35.8	7.7	56.4

It is clear from this table that movements from the private sector in one year, directly to the public sector in the next are rare: only 2.5% of individuals employed in the private sector move to the public sector by the next interview. However, movements from the public to the private sector are considerably more frequent (10.6%). The average annual transition rate into unemployment from the private sector is 2.6% while from the public sector it is around a third of this, 0.9%, suggestive of greater job security in the public sector. This impression is re-enforced looking at the differing probabilities of ever being observed unemployed dependent on the sector that an individual was first observed in: those initally observed in the private sector are subsequently observed in unemployment with a probability of 14.0%, whereas for those first observed in the public sector it is only a 3.4% probability.

Of the unemployed in year *t-1*, 56.4% remain unemployed in year *t*, 35.8% exiting unemployment by transitting into the private sector, 7.7% into the public sector. Of the 402 individuals (17.9% of the sample) ever observed in unemployment during the course of the sample, just under a quarter (24.9%) record being unemployed in three or more consecutive interviews at some point during their observations. These "long-term" unemployed have a much lower probability of exiting unemployment each year (14.5%) than the "short-term" unemployed who find a new job by the next year with a probability of 72.2%. Thus the persistence in unemployment appears to be concentrated on the "long-term" unemployed.

There is also some evidence of public sector attachment, with the probability of an unemployed individual whose most recent employment was in the public sector finding a public sector job, much higher (24.3%) than the corresponding probability for an individual whose most recent employment was in the private sector (5.3%).[37]

4.3.8 *Summary of Descriptive Analysis*

All of the above descriptive analyses help to establish a picture of the sample for each country in our data. There are clearly differences in the labour market structure and dynamics between the countries, however some common patterns emerge:

- In all countries there are differences not only in mean earnings between the public and private sector but also with respect to earnings and job mobility.

- For Germany the data suggests a negative public premium but that returns to experience are greater in the public sector such that the public penalty reduces and is in fact a small positive premium for the middle years of a working life. In contrast, in France there is a positive public premium that initially *falls* with experience, before rising at later years of experience, always remaining a positive public premium. In both the Netherlands and Italy there is a small positive public premium but returns to experience are lower in the public sector such that after just over a decade of experience in each country, the public premium turns negative and remains so for the rest of a working life-time. In Spain the public premium is initially relatively large and positive but reduces in experience and turns negative for the second half of a working life. In Portugal the cross-sectional regressions suggest a small positive public premium for average levels of experience, though the fixed effects estimates find little difference between the sectors.

[37] It has to be borne in mind however that there are only 37 individuals observed unemployed who had most recently been observed in the public sector.

- In almost all countries (Portugal excepted) we find the earnings distribution to be more compressed in the public sector, to a greater or lesser extent, and that there is less earnings mobility in this sector. In Portugal we do find less earnings mobility in the public sector but that it is the private sector that has greater pay compression.

- For all countries, the average annual rate of transition into unemployment is lower for the public sector than it is for the private sector, and in all cases bar the Netherlands the public sector rate is less than half of that of the private sector. The average annual re-employment rates differ according to whether the individual is "long-term" unemployed and in each country there is evidence of public sector attachment, with individuals whose most recent employment was in the public sector having a considerably greater probability of finding a public job in the next period than those who were most recently employed in the private sector.

It clear that in all countries, the public and private sectors differ in cross-sectional earnings levels and both earnings and employment dynamics – all elements that will be important to forward looking agents. As was argued forcefully by Postel-Vinay and Turon (2007), comparisons of cross-sectional income distributions are not very informative in the presence of income mobility. This point is even more pertinent in the case of comparing the income distributions of the public and private sectors, when there are cross sector differences in income mobility. Hence the need in assessing any public-private pay gap to take account of all relevant aspects of the differences between the sectors in order to accurately characterise the difference between employment in the public sector and employment in the private sector.

4.4 A Model of Employment and Wage Dynamics, Between and Within Each Sector

4.4.1 General Structure

In each country, the constructed dataset is a set of N individuals, indexed $i = 1, \ldots N$, each of whom we follow for T_i consecutive years (where $4 \leq T_i \leq 8$, for all i). Each year we observe the individual's job sector (private sector employed, public sector employed, unemployed) and if they are working we observe their monthly earnings. If an individual is unemployed we do not observe his unemployment income, and therefore for the purposes of constructing lifetime expected values we have to impute an unemployment earnings value according to their characteristics and the national unemployment replacement rates[38] more details in section 4.6. We also have information on the individuals time-invariant characteristics, therefore a typical observation for an individual i can be represented by the vector[39] $\mathbf{x}_i = \left(\mathbf{y}_i, \mathbf{S}_i, \mathbf{z}_i^v, z_i^f\right)$, where:

- $\mathbf{y}_i = (y_{i1}, \ldots, y_{iT_i})$ is the observed sequence of individual i's log earnings flows.

- $\mathbf{S}_i = (S_{i1}, \ldots, S_{iT_i})$ is the observed sequence of individual i's labour market states at the various interview dates. We define the three distinct labour market states: employment in the private sector, employment in the public sector and unemployed. S_{it} indicates which of the three states above individual i is in at date t.

- $\mathbf{z}_i^v = \left(z_{i1}^v, \ldots, z_{iT_i}^v\right)$ is a sequence of time-varying individual characteristics. In our application we only consider (polynomials in) potential labour market experience, defined as the current date less the date at which individual i left full-time education.

- Finally, z_i^f is a set of individual fixed characteristics. It includes education level (the 3 ISCED levels) and the year in which the individual first entered the labour market. Hence $\mathbf{z}_i^v$ is deterministic conditional on z_i^f.

In addition to the individual observed heterogeneity as captured by $\mathbf{z}_i^v$ and z_i^f, we allow time-invariant unobserved heterogeneity to influence individual's wages and selection into the various labour market states. The specific form we allow this heterogeneity to take is outlined in section 4.4.2 below, for now we simply append the set k_i of time-invariant unobserved characteristics to the individual's data vector $\mathbf{x}_i$.

We aim to simultaneously estimate transitions between unemployment and employment, transitions between the public and private sector, and earnings trajectories within and between employment sectors. Omitting the parameters that condition the various parts of the model in order to keep the terms concise, we define the individual's contributions to the complete

[38] These rates were obtained from the OECD data portal.
[39] Throughout the paper vectors will be denoted by boldface characters.

likelihood, that is the likelihood of $(\mathbf{x}_i, k_i)$ including unobserved variables, as:

$$\mathcal{L}_i(\mathbf{x}_i, k_i) = \ell_i\left(\mathbf{y}_i \mid \mathbf{S}_i, \mathbf{z}_i^v, z_i^f, k_i\right) \cdot \ell_i\left(\mathbf{S}_i \mid \mathbf{z}_i^v, z_i^f, k_i\right) \cdot \ell_i\left(k_i \mid z_i^f\right) \cdot \ell\left(z_i^f\right). \tag{4.1}$$

Thus the individual likelihood contribution is built from four terms. The last term, $\ell\left(z_i^f\right)$, is straightforward, it is the observed sample distribution of individual characteristics z_i^f. Since $\mathbf{z}_i^v$ is deterministic conditional on z_i^f there is no need for it to feature in this last term. This sample distribution is observed and is independent of any parameter. The penultimate term $\ell\left(k_i \mid z_i^f\right)$, is the distribution of the unobserved individual heterogeneity k_i given observed characteristics z_i^f. The second term is the likelihood of an individual's labour market history given individual heterogeneity, $\ell\left(\mathbf{S}_i \mid \mathbf{z}_i^v, z_i^f, k_i\right)$, with the variable individual characteristics, $\mathbf{z}_i^v$,now explicitly conditioning the labour market history. Finally the first term in the individual likelihood contribution is the likelihood of earnings history given their labour market history and individual heterogeneity, $\ell\left(\mathbf{y}_i \mid \mathbf{S}_i, \mathbf{z}_i^v, z_i^f, k_i\right)$. The first three terms in the individual likelihood depend on various subsets of the model's parameters. We obtain estimates of those parameters by maximizing the sample log-likelihood, $\sum_{i=1}^N \log\left[\int \mathcal{L}_i(\mathbf{x}_i, k_i)\, dk_i\right]$. That is to say, we will deal with the unobserved heterogeneity by integrating the individual's sample log likelihood over the distribution of unobserved heterogeneity in order to determine their likelihood contribution, and then maximise the sample log-likelihood summed over all N individuals. We will now outline the specifics of the modelling of each component of (4.1), to make the methodology clear, beginning with the treatment of unobserved individual heterogeneity.

4.4.2 Unobserved Heterogeneity

In addition to the observed heterogeneity modelled in $\mathcal{L}_i(\mathbf{x}_i, k_i)$ via z_i^f, we consider two types of unobserved heterogeneity: $k_i = (k_i^m, k_i^y)$. The first dimension of this heterogeneity, k_i^m, relates to the individual's propensity to be unemployed or to work in the public sector, therefore captures something of their mobility between sectors (and will be referred to henceforth as their 'mobility class'). The second dimension, k_i^y, refers to heterogeneity in terms of earnings (hereafter referred to as 'wage class') through its impact on both earnings levels and earnings mobility. Both k_i^m and k_i^y are time-invariant random effects which are allowed to be correlated in an arbitrary manner. The mobility class, k_i^m, conditions all the parameters of the model relating to employment and sector history, while the wage class, k_i^y, conditions the parameters relating to earnings history both in terms of levels and mobility. Allowing for different unobserved mobility classes deals with the problem that some people have a higher propensity to work in the public sector (or to be unemployed), hence the selection problem outlined in section 4.3. Moreover, the inclusion of earnings heterogeneity via a time-invariant wage class term helps to capture the persistence in earnings rank, which is not always possible to characterise with fairly low order Markov processes. We refer to mobility and wage *classes* as we employ a finite mixture approach to modelling the unobserved heterogeneity in which each individual can belong to one of K^m mobility classes and K^y wage classes[40]. Therefore in total there are $K = K^m \times K^y$ classes. The probability of belonging to a given class depends on the observed individual heterogeneity, z_i^f:

$$\Pr\left\{k_i^m, k_i^y \mid z_i^f\right\} = \Pr\left\{k_i^y \mid k_i^m, z_i^f\right\} \cdot \Pr\left\{k_i^m \mid z_i^f\right\}. \tag{4.2}$$

To be more specific, we model each component of (4.2) as a multinomial logit with K^y and K^m outcomes respectively. All of the details of the model specification are gathered in Appendix C.2.

4.4.3 Labour Market Mobility

The second component of $\mathcal{L}_i(\mathbf{x}_i, k_i)$ in (4.1) concerns the individual's labour market mobility. The transitions between the three labour market states (public sector employed, private sector employed and unemployed) are allowed to depend only on the individual's state in the previous observation and on observed and unobserved characteristics, thus labour market states are modelled as following a conditional first-order Markov chain. It is useful at this point to introduce the indicators e_{it} and pub_{it} which respectively denote the individuals employment state and job sector at the date-t interview. Specifically, $e_{it} = 1$ if i is employed at the date-t interview, 0 if unemployed; pub_{it} is only defined if $e_{it} = 1$, with $\text{pub}_{it} = 1$ if individual i is employed in the public sector, and 0 if he is employed in the private sector. We thus model the complete (within panel) labour market histories in two stages: the probability of employment at the date-t

[40] We implement this approach following Postel-Vinay and Turon (2007), the finite mixture approach providing a tractable method to account for unobserved heterogeneity.

interview ($e_{it} = 1$), given last period sector and individual heterogeneity, and the probability of public sector employment at the date-t interview ($\text{pub}_{it} = 1$), given employment at date-t ($e_{it} = 1$), previous sector and individual heterogeneity. These probabilities are specified as[41]:

$$\Pr\left\{e_{it}, \text{pub}_{it} \mid S_{i,t-1}, z^v_{i,t-1}, z^f_i, k^m_i\right\} = \Pr\left\{e_{it} \mid S_{i,t-1}, z^v_{i,t-1}, z^f_i, k^m_i\right\} \times \left[\Pr\left\{\text{pub}_{it} \mid e_{it}, S_{i,t-1}, z^v_{i,t-1}, z^f_i, k^m_i\right\}\right]^{e_{it}}. \quad (4.3)$$

Both elements of (4.3) are modelled as logits.

Once we know the individual i's labour market state at time $t-1$, $S_{i,t-1}$, their labour market state at the date-t interview, $S_{i,t}$, is fully encapsulated by the pair $(e_{it}, \text{pub}_{it})$ as captured in the following grid:

		pub_{it}	
		0	**1**
	0	$\longleftarrow$ unemployed	$\longrightarrow$
e_{it}			
	1	employed private sector	employed public sector

The joint conditional probability (4.3) characterises the transitions between labour market states and therefore defines $\Pr\left\{S_{it} \mid S_{i,t-1}, z^v_{i,t-1}, z^f_i, k^m_i\right\}$. There is a standard initial conditions problem in as much as we have to specify the distribution of the initial labour market state, S_{i1}, i.e. model the the joint probability of $(e_{i1}, \text{pub}_{i1})$. We allow this joint probability to depend on both observed and unobserved heterogeneity $\left(z^f_i, k^m_i\right)$ and specify it as the product of two conditional logits:

$$\Pr\left\{e_{i1}, \text{pub}_{i1} \mid z^f_i, k^m_i\right\} = \Pr\left\{e_{i1} \mid z^f_i, k^m_i\right\} \cdot \left[\Pr\left\{\text{pub}_{i1} \mid z^f_i, k^m_i\right\}\right]^{e_{it}}. \quad (4.4)$$

Therefore, the contribution to the likelihood of an individual's job mobility trajectory is:

$$\ell_i\left(\mathbf{S}_i \mid \mathbf{z}^v_i, z^f_i, k^m_i\right) = \Pr\left\{S_{i1} \mid z^f_i, k^m_i\right\} \times \prod_{t=2}^{T_i} \Pr\left\{S_{it} \mid S_{i,t-1}, z^v_{i,t-1}, z^f_i, k^m_i\right\}, \quad (4.5)$$

[41]Note the assumption implicit in (4.3) that only the date-$(t-1)$ component of $\mathbf{z}^v_i$—i.e. individual i's potential experience at date $t-1$—enters the set of conditioning variables for job mobility between dates $t-1$ and t.

where the components of the latter product are given by (4.3).

4.4.4 Earnings Process

The first term in $\mathcal{L}_i(\mathbf{x}_i, k_i)$ (equation (4.1)) involves the modelling of individual earnings trajectories. We only have earnings information for individuals who are observed in employment, which means that earnings information is censored for periods in which an individual is unemployed, complicating the likelihood somewhat. We assume log earnings trajectories $\mathbf{y}_i$ to be the realisation of a Markov process of continuous random variables Y_t. Given the limitation of the sample dimensions, both in terms of N and T, the preferred specification for the order of the Markov process is second-order, thus the earnings process of the model will be expounded under this assumption. Though we have up to eight years in each of the panel datasets, and we require only three years of data to identify a second-order Markov process plus an additional year to identify the unobserved heterogeneity, the relatively small N dimension of the data means that a second-order process is the best fit.

Again, so as not to overload the equations, we temporarily omit the conditioning variables and individual index. The likelihood of a given earnings trajectory over T periods can be written as:

$$\ell(\mathbf{y}) = \ell(y_2, y_1) \cdot \prod_{t=3}^{T} \ell(y_t \mid y_{t-1}, y_{t-2}) = \ell(y_2, y_1) \cdot \prod_{t=3}^{T} \frac{\ell(y_t, y_{t-1}, y_{t-2})}{\ell(y_{t-1}, y_{t-2})}. \tag{4.6}$$

Given the assumption that earnings follow and second-order Markov process the likelihood function involves products of bi- or tri-variate densities. We assume that marginal earnings distributions to be Normal, conditional on observed and unobserved individual heterogeneity. Thus both the earnings mean and variance are allowed to depend on **both** observed and unobserved heterogeneity as well as current sector and previous labour market status — recall that earnings are only available for individuals in employment thus current employment status e_{it} is equal to 1 for all individuals who have $\mu(\cdot)$ defined for period t:

$$\begin{aligned} y_{it} \mid \text{pub}_{it}, e_{i,t-1}, z^v_{it}, z^f_i, k^y_i \quad &\sim \quad \mathcal{N}(\mu_{it}, \sigma^2_{it}) \\ \text{with } \mu_{it} = \mu\left(\text{pub}_{it}, e_{i,t-1}, z^v_{it}, z^f_i, k^y_i\right) \quad &\text{and} \quad \sigma_{it} = \sigma\left(\text{pub}_{it}, e_{i,t-1}, z^v_{it}, z^f_i, k^y_i\right), \end{aligned} \tag{4.7}$$

where $\mu(\cdot)$ and $\sigma(\cdot)$ are given functions (see Appendix C.2 for a fully detailed presentation of all our specification assumptions). Now we introduce the normalised log-earnings as $\widetilde{y}_{it} = \frac{y_{it}-\mu_{it}}{\sigma_{it}}$. The triple $(\widetilde{y}_{it}, \widetilde{y}_{i,t-1}, \widetilde{y}_{i,t-2})$ and the pair $(\widetilde{y}_{it}, \widetilde{y}_{i,t-1})$ are Gaussian vectors with covariance matrices $\underline{\tau}_{it}^{(3)}$ and $\underline{\tau}_{it}^{(2)}$ respectively, which we expand as:

$$\underline{\tau}_{it}^{(3)} = \begin{pmatrix} 1 & \tau_{i,t,t-1} & \tau_{i,t,t-2} \\ \tau_{i,t,t-1} & 1 & \tau_{i,t-1,t-2} \\ \tau_{i,t,t-2} & \tau_{i,t-1,t-2} & 1 \end{pmatrix} \quad \text{and} \quad \underline{\tau}_{it}^{(2)} = \begin{pmatrix} 1 & \tau_{i,t,t-1} \\ \tau_{i,t,t-1} & 1 \end{pmatrix}. \tag{4.8}$$

These τ's are individual-specific and are allowed to vary with observed and unobserved heterogeneity and with labour market sector at t, $t-1$ and $t-2$:

$$\begin{aligned} \tau_{i,t,t-1} &= \tau_1\left(\text{pub}_{it}, \text{pub}_{i,t-1}, z_{it}^v, z_i^f, k_i^y\right) \\ \text{and} \quad \tau_{i,t,t-2} &= \tau_2\left(\text{pub}_{it}, \text{pub}_{i,t-1}, \text{pub}_{i,t-2}, z_{it}^v, z_i^f, k_i^y\right). \end{aligned} \tag{4.9}$$

As outlined above, earnings information is only available at date-t if the individual is employed at date-t, therefore $e_{it} = 1$ and $e_{i,t-1} = 1$ for each of the observations used to identify the $\tau_1(\cdot)$ function, hence these employment indicators are not arguments of the function. The $\tau_2(\cdot)$ function requires in addition that $e_{i,t-2} = 1$ in order for that observation to contribute to the identification of the function, therefore neither is $e_{i,t-2}$ an argument of the $\tau_2(\cdot)$ function. Again, $\tau_1(\cdot)$ and $\tau_2(\cdot)$ are functions specified in Appendix C.2.

Now, with the assumptions and notation introduced in equations (4.7) to (4.9), we can specify the likelihood of the typical individual's earnings trajectory $\mathbf{y}_i$ defined in (4.6). This will depend on the earnings information available, as we observe earnings at date-t only for those in employment. If earnings information is available at the date-t interview, then there are four possibilities regarding the presence of earnings information at the date-$t-1$ and date-$t-2$ interviews:

- Case A: date-$(t-1)$: yes; date-$(t-2)$: yes
- Case B: date-$(t-1)$: yes; date-$(t-2)$: no
- Case C: date-$(t-1)$: no; date-$(t-2)$: yes
- Case D: date-$(t-1)$: no; date-$(t-2)$: no

Which case an individual's date-t observation is will determine whether that term in the product in (4.6) is $\ell(y_t \mid y_{t-1}, y_{t-2})$, $\ell(y_t \mid y_{t-1})$, $\ell(y_t \mid y_{t-2})$ or simply $\ell(y_t)$.

In the complete earnings information scenario, an individual has case (A) for all $t >= 3$, that is they have earnings information not only at the date-t interview but also, $(t-1)$ **and** $(t-2)$ interviews, for all $t >= 3$. In this scenario, the equation (4.6) earnings trajectory simplifies neatly to become:

$$\ell_i\left(\mathbf{y}_i \mid \mathbf{e}_i, \mathbf{pub}_i, \mathbf{z}_i^v, z_i^f, k_i^y\right) = \left[\prod_{t=1}^{T} \frac{1}{\sigma_{it}}\right] \times \left[\frac{\prod_{t=3}^{T} \varphi_3\left(\widetilde{y}_{it}, \widetilde{y}_{i,t-1}, \widetilde{y}_{i,t-2}; \underline{\mathcal{I}}_{it}^{(3)}\right)}{\prod_{t=3}^{T-1} \varphi_2\left(\widetilde{y}_{it}, \widetilde{y}_{i,t-1}; \underline{\mathcal{I}}_{it}^{(2)}\right)}\right], \tag{4.10}$$

where $\varphi_n\left(\cdot; \underline{\mathcal{I}}^{(n)}\right)$ is the n-variate normal pdf with mean 0 and covariance matrix $\underline{\mathcal{I}}^{(n)}$. However, given that in each country, individuals move between employment and unemployment through the course of the panel (indeed we would not be able to identify the mobility parameters were they not to) it is necessarily not possible that we have case A for all individuals for all $t >= 3$. In the more general case, the individual's trajectory is built from a product that cannot be captured in as elegant an expression as equation (4.10). In each case however the individual's earnings trajectory likelihood is built from products of uni-, bi- and tri-variate normal densities and has the general form of equation (4.6).

For a case (B) observation at date-t, an individual has earnings information at date-t and also at date-$(t-1)$ but **not** at date-$(t-2)$. In this case the likelihood of that observation is computed as:

$$\ell(y_t \mid y_{t-1}) = \frac{\ell(y_t, y_{t-1})}{\ell(y_{t-1})} = \frac{1}{\sigma_t} \times \frac{\varphi_2\left(\widetilde{y}_t, \widetilde{y}_{t-1}; \underline{\mathcal{I}}_t^{(2)}\right)}{\varphi(\widetilde{y}_{t-1})} \tag{4.11}$$

where $\varphi(.)$ is the univariate standard normal pdf.

Similarly for a case (C) observation at date-t, an individual has earnings information at date-t and also at date-$(t-2)$ but **not** at date-$(t-1)$. In this case the likelihood of that observation is computed as:

$$\ell(y_t \mid y_{t-2}) = \frac{\ell(y_t, y_{t-2})}{\ell(y_{t-2})} = \frac{1}{\sigma_t} \times \frac{\varphi_2\left(\widetilde{y}_t, \widetilde{y}_{t-2}; \underline{\mathcal{I}}_t^{(2)}\right)}{\varphi(\widetilde{y}_{t-2})} \tag{4.12}$$

Finally, for a case (D) observation, the individual has earnings information only at the date-t

interview, in which case the likelihood of the earnings observation is computed straightforwardly as:

$$\ell\left(y_t\right) = \frac{1}{\sigma_t} \times \varphi\left(\widetilde{y}_t\right) \tag{4.13}$$

Clearly, for all individuals their first earnings observation is a case (D), and if they are observed employed in the following year that observation will be a case (B) observation.

We are effectively assuming that normalised log earnings follow a familiar AR(2) process, though we build in more flexibility than a standard second-order Markov process in that we allow the τs to depend on observed (and unobserved) individual characteristics in (4.9). This has the dual appeal of (a) helping to more accurately fit the observed mobility of income ranks, and (b) informing one of the key questions that we aim to address: namely how income mobility varies across individuals and across sectors. The τs offer an index of income mobility which we will use to shed light on this key question.

It is acknowledged that an implicit assumption of the model as outlined above, is that transitory shocks to the earnings process are independent of the transitory shocks to the processes determining mobility between the labour market sectors. To put this another way, we assume that the individual earnings process only affects individual mobility between states through either observed characteristics (e.g. education and experience) or through the time-invariant unobserved individual random effects k_i^m and k_i^y, and not through any transitory (unobserved) shocks. This assumption leads to the separability of the likelihood function into a part relating to labour market mobility and a separate part relating to the earnings process.

4.4.5 Likelihood Maximisation

Having established the specifications for the individual contributions to the complete likelihood, $\mathcal{L}_i\left(\mathbf{x}_i, k_i\right)$ defined above, the parameter estimates are obtained by maximisation of the sample log-likelihood:

$$\sum_{i=1}^{N} \log\left(\sum_{k_i^m=1}^{K^m}\sum_{k_i^y=1}^{K^y} \mathcal{L}_i\left[\mathbf{x}_i, \left(k_i^m, k_i^y\right)\right]\right), \tag{4.14}$$

where as touched on above, the individual random effects $k_i = \left(k_i^m, k_i^y\right)$ are integrated out of the complete likelihood (4.1).

Given that this sample log-likelihood is highly non-linear and that there is a large number of parameters to be estimated, maximisation cannot be completed in a single step. We proceed by employing a sequential version of the EM algorithm. The advantage of this procedure is that it is computationally more stable given arbitrary starting values and is more tractable than a direct frontal maximisation of the total sample likelihood (4.14). Furthermore, it can be shown that under the assumptions of identification of the model parameters and numerical convergence of the algorithm, that the two stage approach converges to a consistent estimator of the parameters (see Bonhomme and Robin, 2004). It does have the drawback in that it converges to an estimator which differs from the maximum-likelihood estimator and is not efficient – for two reasons: firstly because it is a two-step procedure, and secondly because of the sequential nature of the optimisation we do not use all of the information and thus lose some efficiency. Appendix C.4 details the EM algorithm procedure.

4.5 Results

The results of the model estimation for each country are now presented. Firstly, we present an assessment of the fit of the model for each country, in order to establish that the model does a good job of replicating cross-sectional earnings statistics in each country, and also importantly that it fits the dynamics of the labour market and earnings well. Following this the results in each country are separately presented. Subsequently section 4.6 will develop a more systematic analysis of selection into the public sector and the differences between the sectors in terms of earnings flows and lifetime values. The model is estimated under the assumption that within each country, individual unobserved heterogeneity can be modelled with three mobility classes and two wage classes. We were guided in the choice of these numbers by balancing the various concerns of descriptive accuracy, computational tractability and model fit. In addition, we were keen to adopt a common framework that could be applied equally to all countries. The common model for all countries comes at a certain price however, in that the model has to give a good fit to each and every country. We believe that three mobility classes and two wage classes provides the best solution given these competing concerns.

This gives us six classes in total, which means that for each country there are 86 parameters to estimate[42]. Tables of the coefficient estimates and standard errors are reported in Appendix

[42]Of these 86, 48 relate to the mobility estimation, 38 relate to the earnings estimation

C.3. However, rather than commenting on six countries times 86 coefficients directly, we chose in the subsequent results sections to concentrate on more easily interpreted statistics – such as the predicted mean earnings and predicted τs – that can illuminate the various components of the model.

4.5.1 *Model Fit*

We begin the discussion of the model results by examining how well the statistical model of section (4.4) fits the data and captures the dynamics of labour market states and earnings in each country. In order to do this, we simulate the model in each country and then compare the model-generated data outcomes with the real data. To achieve this, within each country, we replicate our panel 6 times (i.e. as many times as there are unobserved heterogeneity classes in total) so that we have one set of observations per person per unobserved heterogeneity class. We use the estimated job mobility and earnings processes to simulate individual labour market and earnings trajectories for each (individual$\times$class) in the sample. We then produce simulated descriptive statistics, weighting each $\{\text{individual } i, \text{class } (k^m, k^y)\}$ observation by the probability that individual i belongs to mobility class k^m and wage class k^y, given individual's observed characteristics $\mathbf{x}_i$: $\Pr\{k_i^m = k^m, k_i^y = k^y \mid \mathbf{x}_i\}$.

Worker Allocation and Mobility Between States

Looking at the cross-sectional statistics for all countries, it seems that the model fits the observed pattern of worker allocation to states – private sector employment, public sector employment and unemployment – very well indeed. Tables 4.25, 4.28, 4.31, 4.34, 4.37 and 4.40 compare the sectoral composition of classes, and the class composition of sectors, for the observed and model-generated data, illustrating this very good fit for each country[43]. Moreover, as the left panel of each table shows, for each country it is the case that in the real data there are non-trivial proportions of the population in each of the three mobility classes, suggesting that three unobserved heterogeneity types is a good approximation to reality.

As our main concern in this paper is to examine the differences between the public and

[43]There is a discrepancy for the Netherlands between the predicted and observed proportion of the unemployed who are $k^m = 1$ or $k^m = 3$, however, the unemployed represent only 2.7% (386) of the sample observations, making it difficult to perfectly fit the three different unobserved mobility classes on so few individuals.

private sectors, taking into account selection into these sectors and the different job loss rates and earnings dynamics between the sectors, it is of primary importance that we fit the dynamics of labour mobility well. Tables 4.13 to 4.18 illustrate the observed and simulated cross-job-state transition matrices for each of the countries at intervals of one and five years[44]. As discussed above, one of the criteria in deciding the model specification was to have a common statistical model that could be applied to all countries and fit well. As such, the specification chosen fits the observed job mobility better in some countries than in others. Looking at the upper panel of each table, we can see that transitions from $t-1$ to t are generally fitted very well, in all cases the maximum absolute distance between the observed rate and the model-predicted rate being less than 10%-points and in most cases much less than that. In addition to the maximum distance between the observed and predicted figures in any of the nine entries in each 3×3 matrix, we report the maximum absolute distance between the observed and predicted figures relating to the 2×2 matrices formed by excluding the unemployment column and row of each matrix. This shows how well the model is fitting persistence in sector for those employed, and the movement between them. In each case, for the $t-1$ to t transitions, we fit these 2×2 matrices very well, the error being of the order of around 1%-point for three of the countries and more towards 2%-points for the other three. This shows that we are fitting the employment sector persistence well in all countries; the prediction errors are due to an under-prediction of unemployment persistence to a greater (France, Netherlands, Portugal) or lesser (Germany, Spain, Italy) extent. Specifically, we under predict the unemployment persistence because we are over-predicting the rate of re-employment into the private sector – the rate of re-employment into the public sector is fitted well in each country.

Looking at the longer-lag transitions, from $t-5$ to t, it is clear that the under-prediction of unemployment persistence continues to be a problem in France in particular and now Germany. In the other countries the maximum distance between predicted and observed figures ranges from 3.1%-points (Spain) to 7.5%-points (Portugal), which whilst not perfect show that the fit of the model does not deteriorate too much at longer lags[45]. Two comments: the poorer fit at

[44]With up to 8 observations for some individuals in each dataset, in theory we could look at 7-year lags for each country, however as there are relatively small numbers of individuals who have 8 observations, the cell sizes in the predicted data preclude robust observed matrices at longer than 5 lags.

[45]In fact for the Netherlands and Spain the maximum distance in the 3×3 matrices is smaller for the longer lag than the 1-period lag.

the longer lag for France and Germany, is partly caused by the small numbers of individuals in the observed data who are unemployed at the $t-5$ period, just 126 in the case of France (290 Germany) which is five times fewer (four times fewer for Germany) than the number observed unemployed in the $t-1$ period for the upper panel calculations. This inevitably makes it more difficult to fit the transitions accurately when the observed figures are calculated from relatively small cell sizes. Secondly, for France, Germany, Italy and Spain, the 2×2 matrices distances continue to be small, of the order of 2 to 3%-points, indicating that we are fitting the persistence in and movement between sectors for employees pretty well. For the Netherlands (6.5%-points) and Portugal (7.5%-points) we predict unemployment persistence reasonably well at the longer lag but slightly under-predict the movement between the public and private sectors. In sum, these Tables indicate that the statistical model does a satisfactory job in all countries of fitting the observed transitions between the labour market states, both at short and longer lags, and in some countries in particular (Italy, Spain) does an very good job indeed.

Earnings Dispersion and Earnings Mobility

For our approach to the assessment of public-private differences over the lifetime to succeed, the other aspect which we need to fit well in each country is the cross-sectional earnings profile and the mobility of earnings. Concentrating initially on the former, Figures 4.7 to 4.12 plot the observed and predicted log earnings densities for the whole sample and also for the private and public sectors separately. These figures also include the wage class-specific densities, in each case normalised at the relative size of each class within each sector. In our model specification we settled on having two wage classes, and as can be seen in the upper panel of each of the Tables 4.26, 4.29, 4.32, 4.35, 4.38 and 4.41, within each country there is a substantial proportion of individuals in each class. For Germany, the Netherlands and France there is an even distribution between the classes whereas for Italy, Spain and Portugal one class has notably more weight than the other.

Looking first at the total picture for each country, bottom left panel of each figure, we can see that in each country the model fits the observed wage distribution well – though again, given the common model specification across all countries, inevitably the fit is better in some countries (Germany, Netherlands, Portugal, Italy) than it is in others (France, Spain). This suggests

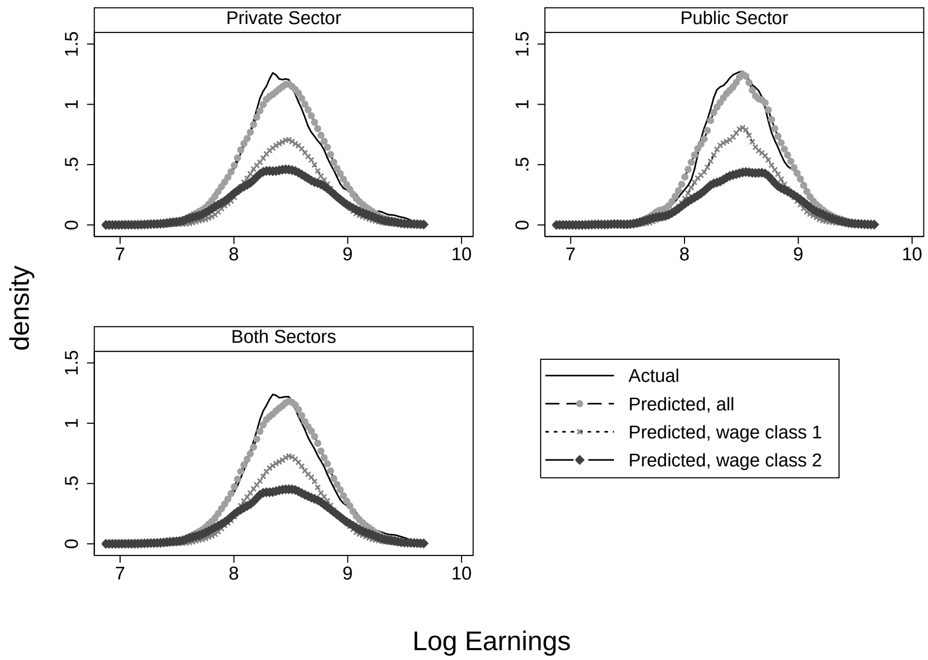


Figure 4.7: **Germany**: Earnings Densities

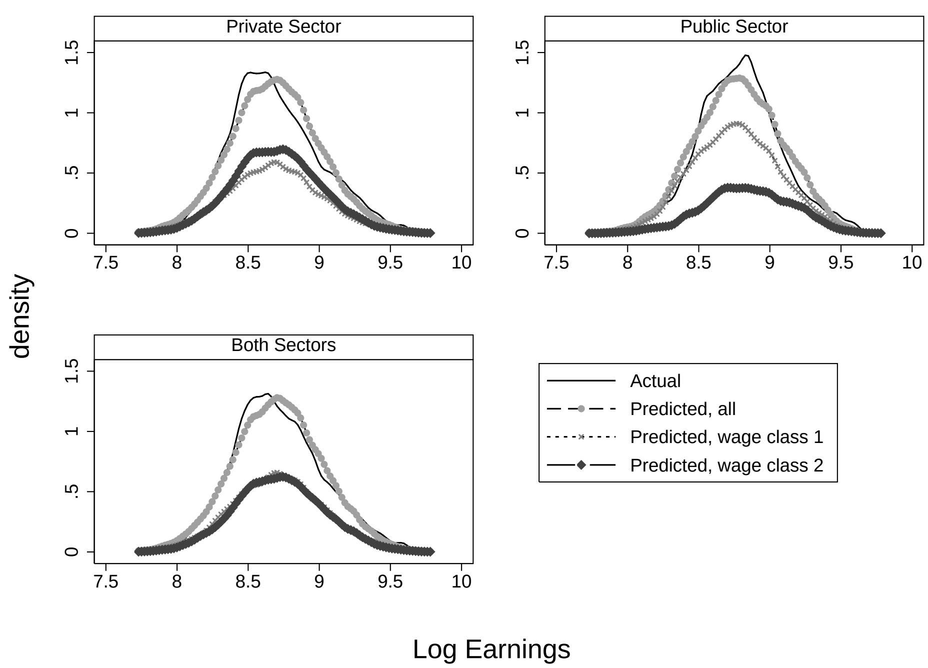


Figure 4.8: **Netherlands**: Earnings Densities

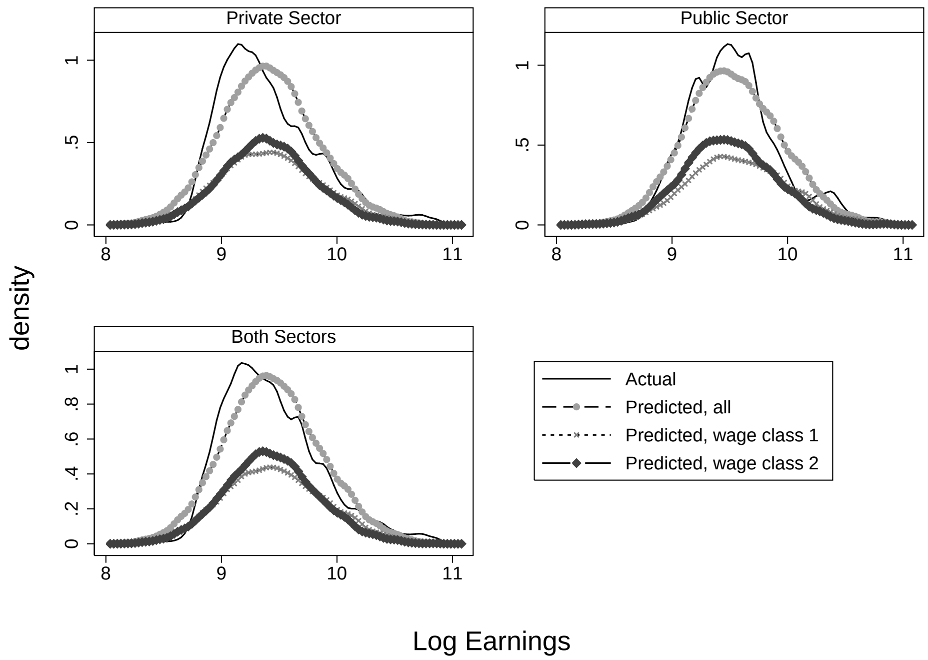


Figure 4.9: **France**: Earnings Densities

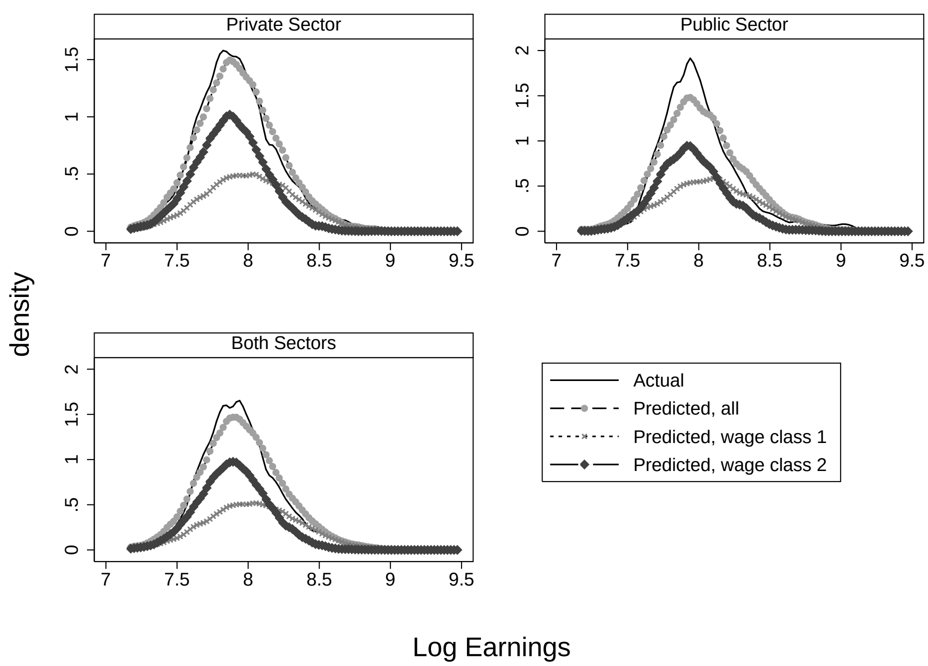


Figure 4.10: **Italy**: Earnings Densities

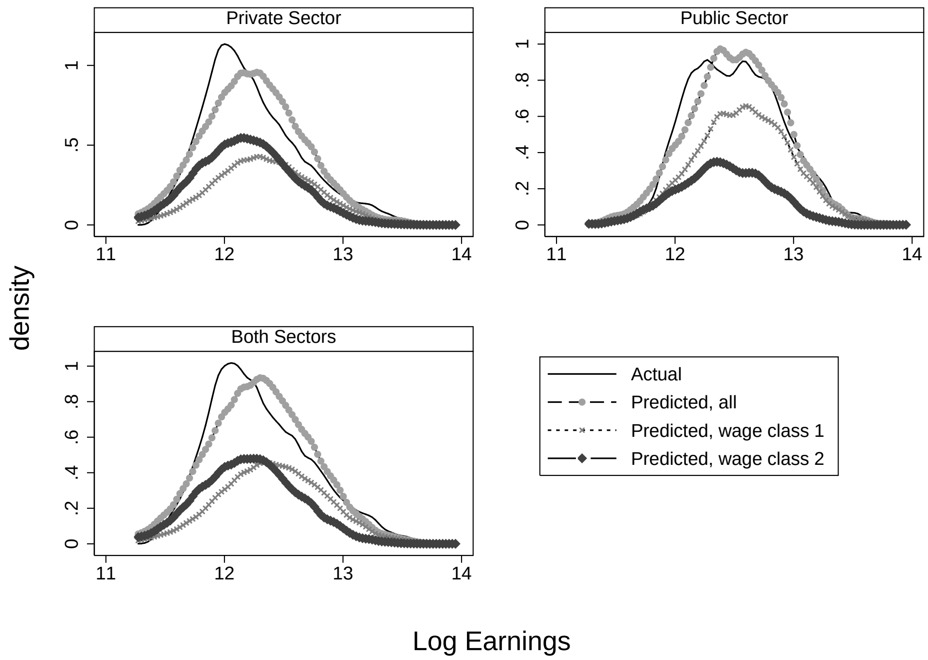


Figure 4.11: **Spain**: Earnings Densities

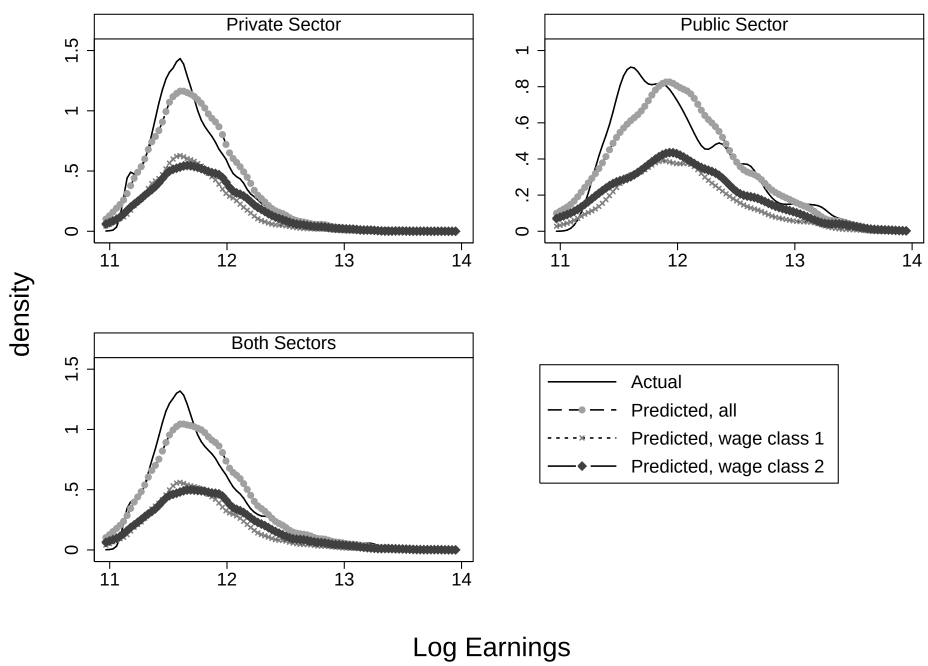


Figure 4.12: **Portugal**: Earnings Densities

that the mixture of two normal densities in each country fits the observed wage density in each country pretty well. The wage class-specific densities illustrate both the prevalence of each wage type in each country, through the relative size of that class's density, and also how the wage classes within a country relate to each other in terms of earnings mean and distribution. Still concentrating on the overall pictures for each country, it is clear that while in some countries overall the wage classes have similarly shaped earnings distributions (Germany, Netherlands, Portugal), for other countries (Italy and Spain) one wage class clearly has higher earnings than the other. Turning now to the sector-specific densities, we can see that where the overall fit is good the sector-specific fits are also good. For France and Spain the public sector fit is better than the private sector fit, and in no case is there a large discrepancy between the observed and predicted densities. We can also observe that within countries, there are differences in the public and private sector densities between the wage classes. For example in Italy, wage class 1 has a higher mean than wage class 2, and we can see from the sector-specific densities that wage class 1 types in either sector earn more on average and have a less skewed and peaked distribution. It is a similar story for Spain. Overall the fit of cross-sectional earnings densities is arguably satisfactory for all countries.

Again, for the purposes of our analysis, it is important not only to fit the cross sectional distributions of earnings well but also the mobility of earnings. Above we compared the observed job mobility in the sample with simulated job mobility using the coefficients generated by the statistical model. When we performed these simulations, we simulated full individual labour market histories – i.e. featuring both earnings *and* job state transitions – with earnings evolving according to the process outlined in the specification. We can therefore compare the predicted earnings quintile transition matrices with those obtained from the real data. Again we do this at lags of both one and five years, see Tables 4.19 to 4.24.

Concentrating firstly on the 1-period transition matrices (upper panel of each table), the maximum discrepancy across all countries and cells of the matrices is 9.7%-points, the minimum largest discrepancy being 4.0%-points. Given the relatively parsimonious specification of income means, variances and covariances, and that we have only six unobserved heterogeneity classes in total, this is a very good fit. Moreover, as we move to longer lags, (lower panel of each table), though there is an increase across the board in the maximum absolute distance between the observed and predicted figures, the increases are not large for most of the countries. France and Spain are exceptions to this. For France, the difference as we move from one lag to five lags is an increase in maximum distance from 4.0%-points – the lowest for 1-lag – to 11.8%-points. Though this increase is large relative to the one-period lag, it is still a reasonable fit at the longer lag. With respect to Spain, the five-lag maximum distance of 14.7% is slightly larger than we would hope, but relative to the one-period lag is not actually that much of a loss in precision – the model does less well at fitting earnings mobility in Spain at all lags, though arguably it is still a satisfactory fit.

Taken in consideration with the fit of cross-sectional job sector, job sector mobility and the cross-sectional earnings distribution, the earnings mobility fit of the model suggests that for each country our statistical model does a good job of capturing the observed levels and dynamics in each country and supports the specification. This is pleasing given that we have estimated the same specification on each and every country, with a small number of latent heterogeneity classes and only a second order Markov earnings process.

As already alluded to, the choice of specification involved balancing competing criteria and was constrained by the wish to estimate a common model for all countries, and thus having to develop a model that could fit the country with the smallest $N \times T$ dimensions as well as the largest.

Possible Alternative Specifications

It is clear from the observed data that earnings are highly persistent in each country, and the assumptions of our model give two mechanisms through which this persistence is captured: the 2^{nd}-order Markov process for the evolution of earnings, and the time-invariant unobserved wage classes. The combination of these assumptions goes a long way to capturing the observed persistence in each country. However, if we look at the prediction errors for each country, in Tables 4.19 to 4.24, we see that for both the one-period earnings transitions and the five-period transitions, the model in general *under*-predicts the persistence in earnings. For some countries, persistence in the lowest quintile(s) of earnings is actually over-predicted, especially at the longer time lag, however the majority of cells in the main diagonal of each country's matrices are under-predicted by the model – indicating that we *over*-predict earnings mobility to some extent in each country. This aspect of the model could potentially be improved by altering the two assumptions relating to the earnings process, either by increasing the order of the Markov earnings process or by increasing the number of latent earnings heterogeneity classes. However, given the nature of our estimation procedure, the computational cost of expanding the model in either of these directions is very high. There is a trade-off between the amount of "built-in" persistence resulting from the order of the Markov process, and the additional earnings auto-correlation introduced by the time-invariant unobserved earnings classes. The choice we made of a 2^{nd}-order Markov process with two wage classes was guided, as ever, by a number of competing concerns including computational tractability, parsimony, model fit and the aim to estimate the same model specification for each country. Given these concerns, the model specification has to be guided by the $N \times T$ dimensions of each of the datasets we have: in each country we have a relatively small N dimension – between 2213 (Netherlands) and 3026 (Germany) individuals – balanced by a longer T dimension – each individual having at least 4 and up to 8 observations. Increasing the order of the Markov process beyond 2 would require each individual to have at least 5 observations, which would further cut the sample size in each case. Given the restrictions imposed by the estimation samples, a specification with a 2^{nd}-order Markov process is the most appropriate. In terms of the number of unobserved earnings heterogeneity classes, again the relatively small N dimensions of the datasets dictates that two wage classes gives the best fit. It is natural given three labour market states – private, public, unemployed – to have three mobility classes, thus adding an additional wage class consequently increases the total number of unobserved classes by 3, quickly testing the limits of the data[46]. The current specification is the compromise that best meets the various competing considerations.

There are a number of possible alternative strategies that are computationally tractable. For example, removing the unobserved earnings classes altogether would provide a model that is computationally quick and easy to estimate, however in testing of various model formats Postel-Vinay and Turon (2007), consistently found that such models grossly over-predict both job and earnings mobility at lags beyond one or two years. Similarly, restoring the assumption

[46]The N/T ratio of the dataset plays a critical role in determining the best specification. For example, Postel-Vinay and Turon (2007) had a larger N and T dimension, and were able to get a very good fit with a 2^{nd}-order Markov process, 3 latent wage classes and 3 latent mobility classes; whereas Bonhomme and Robin (2004) used similar estimation techniques but were constrained to a 1^{st}-order Markov process as the T-dimension of their data was only 3 years, but balanced by a very large N dimension.

of unobserved earnings classes but reducing the order of the Markov process for earnings to just 1^{st}-order is simpler and quicker to estimate, but again results in substantially larger prediction errors – as compared with the 2^{nd}-order process – at the longer lags. Given that the purpose of our paper is to use the model to construct the lifetime values of individual labour market trajectories, having as good a fit as possible of the earnings mobility is extremely important. Thus the specification using a 2^{nd}-order Markov process, 2 time-invariant unobserved wage classes and 3 time-invariant unobserved mobility classes, appears to be the right compromise for our purposes.

We next present the results of the model for each country, focusing first on the distribution of mobility types across labour market states and the differing predicted job loss rates between the sectors. We then look at the earnings results of the model, assessing the existence and magnitude of any public premium in earnings, controlling for selection effects. We do this by predicting earnings for each individual in each sector given their observed and unobserved characteristics and the coefficients estimated from our statistical model.

4.5.2 *Germany*

Labour Market States

Looking at the left ('Observed') panel of Table 4.25, we see that just over half of the sample are mobility class $k^m = 2$ types, with the remainder even split between $k^m = 1$ and $k^m = 3$. Selection into labour market state is very much correlated with mobility type, with $k^m = 2$ (resp. $k^m = 3$) types selecting overwhelmingly into the private (resp. public) sector, while $k^m = 1$ is a mixture of mainly private sector workers, though with a higher unemployment rate among this type than the other two. For Germany, we will refer to the $k^m = 1$ types as 'high unemployment', $k^m = 2$ as 'private worker' and $k^m = 3$ as 'public worker' types.

The upper panel of Table 4.26 shows the human capital characteristics of each mobility type. Following the descriptive analysis in which we found that the public sector attracted more high educated and slightly more experienced workers, we similarly find that compared to the 'private worker' type, the 'public worker' type have a higher proportion of high education workers (37.2%

versus 28.4%) and slightly more experience (18.0 years versus 16.6). The 'high unemployment' type have higher than average experience (20.5 years) but substantially lower education than the other types (only 14.8% are high education).

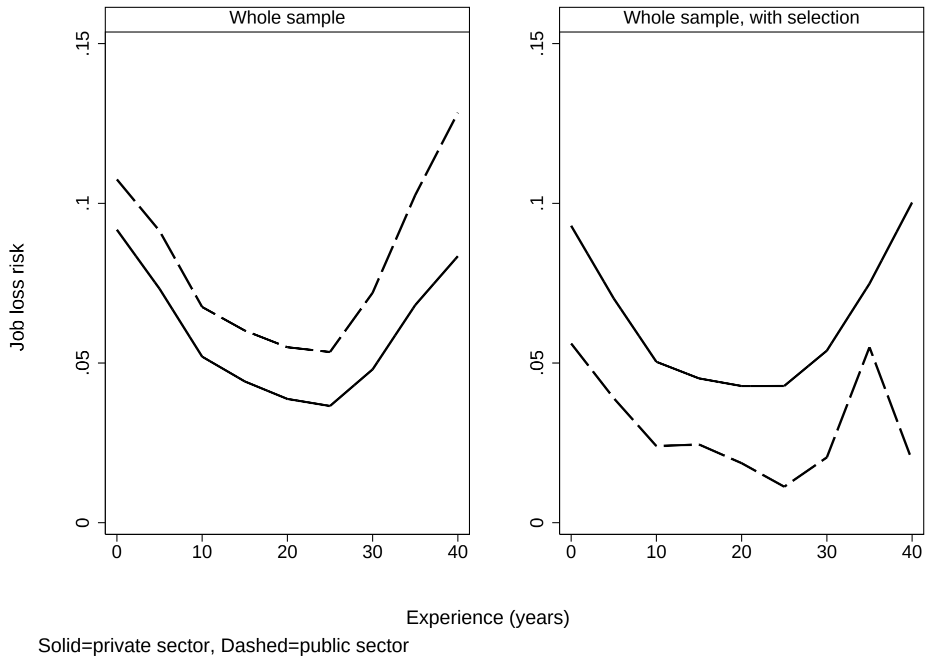


Figure 4.13: **Germany**: Job Loss Risk

Figure 4.13 illustrates the difference in predicted job loss risk for each sector against experience. The left-hand "whole sample" panel is clean of selection effects as we calculate these average predicted job loss rates at each year of experience assuming each individual is in each sector. For each sector the job loss risk traces a U-shape in experience, rising from 25 years of experience onwards. Interestingly the job loss risk is higher in the public than the private sector for all levels of experience. When we allow for selection – calculating the job loss probability for each sector using only individuals observed to be in that sector in the first period of the data – while the private risk remains unchanged, there is a large downward shift of the public sector job loss risk, it approximately halves for almost all levels of experience. This suggests that there is positive selection into the public sector, with higher employment individuals selecting into this sector. We know that the public sector attracts more educated and experienced individuals, and that the mobility type that predominate in the public sector ($k^m = 3$) have low unemployment rates (as seen in Table 4.25) and this affects the unemployment probabilities in the selected sample.

Earnings

In Germany the sample individuals divide pretty evenly between the earnings classes, 52.5% into $k^y = 1$, 47.5% into $k^y = 2$. Table 4.27 shows that the wage class 1 earn more on average than wage class 2 in both the private sector (+2 log points more) and the public sector (+1 log point more). Wage class 1 have a small negative public premium (−1 log point), whereas on average wage class 2 earn the same in each sector. The most notable difference between the classes is that compared with the $k^y = 2$ types, the $k^y = 1$ types have much lower dispersion of earnings in each sector (8 log points lower in each sector).

The upper panels of Figure 4.14 plot mean earnings along with the 10^{th} and 90^{th} percentiles of earnings, against experience for each wage class and sector. The bottom left panel shows the same profile but for the whole sample, when we control for selection effects i.e. we include each sample member in each sector and therefore compare the cross-sectional profiles in *potential* earnings in each sector. The bottom right panel shows the picture when we only include in the private sector the predicted private sector earnings of an individual who is observed in the real data to be in the private sector in the initial period; the corresponding public sector distribution is constructed using the same logic. In this way we allow for selection of individuals into sectors.

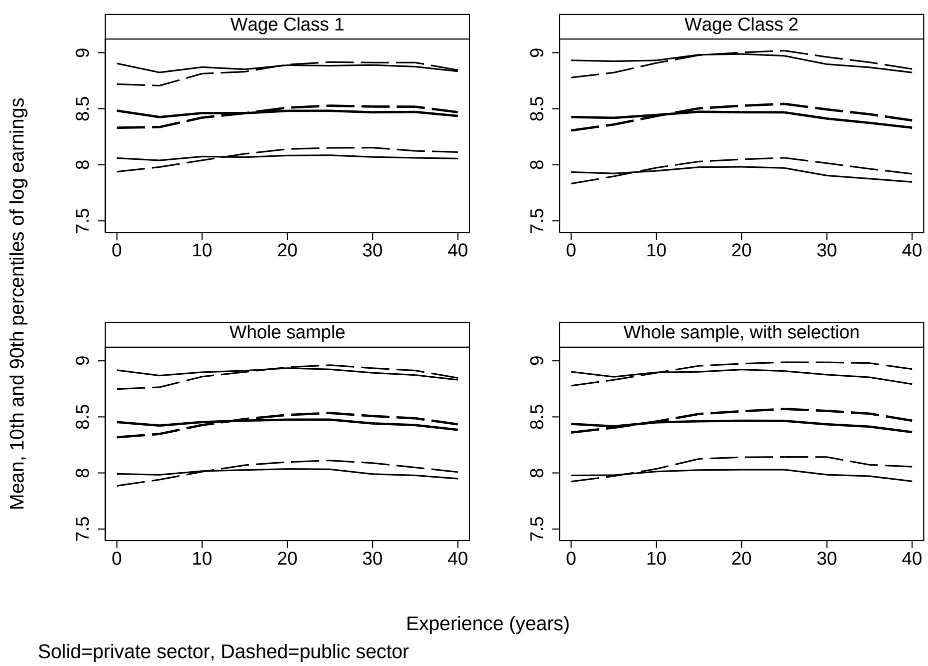


Figure 4.14: **Germany**: Earnings-Experience Profiles

The first thing to notice about these plots is the difference in returns to experience between the sectors, or more specifically how flat the experience profiles are for the private sector for both wage classes. For each class the public profiles are much steeper initially before levelling off after approximately 20 years of work experience and then tapering off towards the end of the working lifetime. Interestingly the dispersion of wages does not seem to be dependent on the experience level, the 10^{th} and 90^{th} percentile lines maintaining their relative positions over the years in all panels. We can see the lower dispersion of earnings in each sector for wage class 1 compared with wage class 2 from the relative closeness of the mean, 10^{th} and 90^{th} percentiles in the top left panel, compared with the top right.

The difference in public premium between the classes is not very large (-1 log point for $k^y = 1$ compared with no premium for $k^y = 2$), and for each wage class the reduction in dispersion is approximately the same (-1 log point reduction in the standard deviation of earnings).

Turning to the bottom left panel of the figure, the average potential public premium controlling for selection is -0.8 log points, with a reduction in earnings standard deviation in the public sector also of 1.2 log points, tallying with what we find for each class. This fits with the finding of Dustmann and van Soest (1998), who find a negative public premium reducing in experience. The average public premium when we do not control for selection, bottom right panel, is larger at 4.7 log points (similar to the finding in Table 4.1). Looking at the bottom two panels of the figure illustrates that non-random selection into sector is important in explaining the public premium: there is positive selection into the public sector which explains the observed raw positive public premium, and why it disappears when selection is controlled.

One measure of earnings mobility is given by the 1-lag auto-covariance of earnings disturbances (the τ_{it}^1 coefficient in the statistical model), and this is plotted against experience for each sector$\times$wage class in Figure 4.15.

For each wage class we can see that earnings are more persistent in the public than the private sector, and that in each sector earnings persistence increases with experience. This is particularly the case for the $k^y = 1$ types who have a rapid increase in earnings persistence, regardless of sector, during the first 10 years of work, before this growth in persistence slows and grows at a similar more modest rate as in wage class 2. The $k^y = 2$ types have a greater difference in persistence between the sectors, and this difference remains constant in experience;

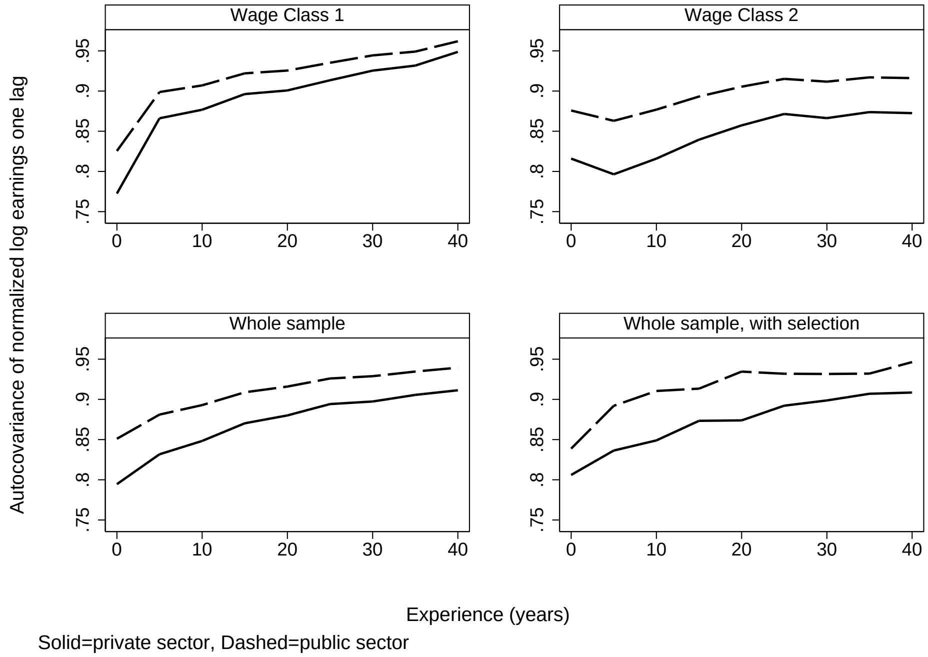


Figure 4.15: **Germany**: Auto-covariance of Normalised Earnings

similarly for wage class 1 the difference in persistence between the sectors does not alter with experience. Given that the sample is split fairly evenly between the wage classes, the picture for the sample as a whole is a mixture of the two upper panels, with greater persistence in the public sector, increasing with experience. The similarity between the lower left and right panels indicates that selection does not have a discernible impact on this metric of earnings persistence.

The bottom panel of Table 4.26 shows the breakdown of observable characteristics for each wage class. Given that the difference in earnings between the classes is not that substantial it is unsurprising to find that in terms of education and experience, each class has a similar composition. The slightly higher earning class ($k^y = 1$) actually have lower education on average, though this appears to be offset by more experience. The lower panel of this table also shows the joint distribution of unobserved heterogeneity classes. Each of the $K^m \times K^y = 6$ classes contain a non-negligible proportion of the sample, so all combinations are found in the data. Nevertheless, the two dimensions of unobserved heterogeneity are not independent, for example, the $k^y = 1$ wage class have the higher private earnings, and are over-represented among the 'private worker' mobility type $k^m = 2$.

4.5.3 *Netherlands*

Labour Market States

The 'Observed' panel of Table 4.28 shows the sectoral composition of the mobility types and the mobility composition of the sectors for the Netherlands. Almost two-thirds of the sample are captured by mobility type 3, with an even split of the remainder between types 1 and 2. It is clear that mobility type has a large correlation with labour market state: this largest type $k^m = 3$ is almost totally private sector employed (98.31%) with the rest unemployed, no public sector at all (henceforth referred to as the 'private worker' type). Similarly type $k^m = 1$ is even more exclusively public sector, with less than 1% in unemployment and no private sector workers (referred to as the 'public worker' type). The $k^m = 2$ types collect those not so attached to either sector, and has by far the highest unemployment rate of all of the types (referred to as the 'high unemployment' type).

In terms of the observable characteristics of each mobility type, given the comprehensive sorting of types into sectors, unsurprisingly we find that the education and experience characteristics of the 'private worker' and 'public worker' types, mirror very closely the breakdowns by sector outlined in section 4.3.3, with the 'public' types having a significantly higher (lower) proportion of high (low) education workers, and having more potential labour market experience.

For the Netherlands, the unemployment rate is low and in each sector and for each the job loss risk traces a U-shape in experience, as illustrated in the "whole sample" panel of Figure 4.16. The job loss risk is very similar in each sector controlling for selection, higher for the public sector at the very lowest and highest levels of experience but lower through the majority of the working lifetime. Comparing this picture with the right-side panel, which allows selection into sectors, we see that selection does not have a substantial effect on the job loss risk for either sector, aside from reducing the public risk at higher levels of experience.

Earnings

For the Netherlands there is a fairly even split in the sample between the earnings classes, 51.8% into $k^y = 1$, 48.2% into $k^y = 2$. From Table 4.30 we see that wage class 1 types earn more on average than wage class 2 in the private sector (8.70 versus 8.69) but have a -9 log point public

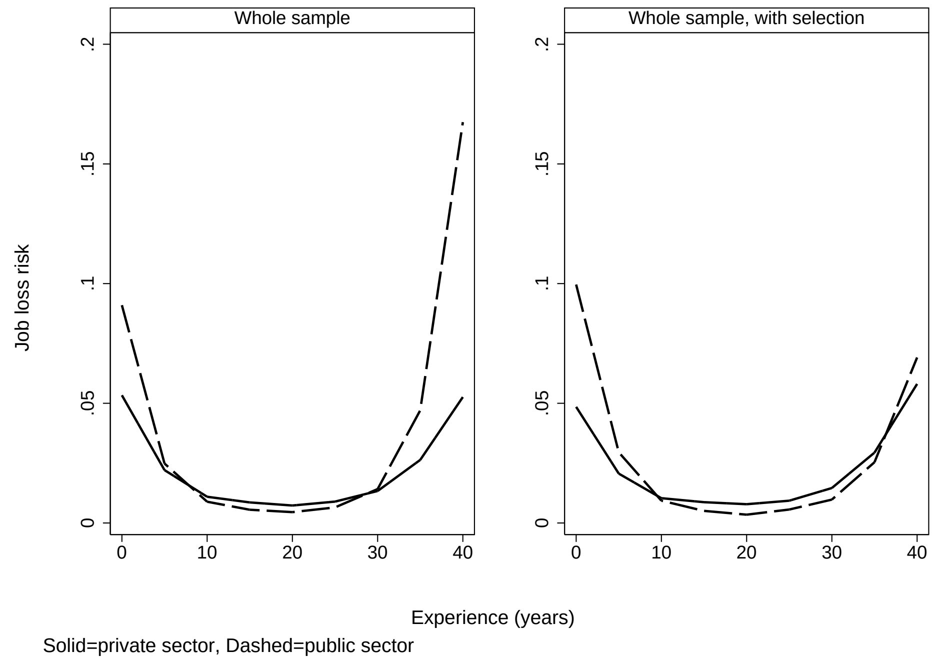


Figure 4.16: **Netherlands**: Job Loss Risk

premium, while the wage class 2 has an +4 log point public premium on average, such that this class earns on average 12 log points more in the public sector than wage class 1. For each wage class there is a lower standard deviation of log earnings in the public sector, by 2 to 3 log points.

Again, Figure 4.17 plots mean earnings, the 10^{th} and 90^{th} percentiles of earnings, against experience for each wage class and sector separately, and for the "whole sample" and the "whole sample, with selection".

The figure visually confirms the finding of Table 4.30, that for wage class 1 there is a negative public premium while for wage class 2 it is a positive public premium. Moreover, the figure illustrates that for wage class 1, the negative public premium is evident not just at the mean but at both the 10^{th} and 90^{th} percentiles also. For wage class 2 there is a premium in the mean at all levels of experience, and at the 10^{th} percentile, while for the 90^{th} percentile the premium is only evident at higher experience levels. For each sector and class, the dispersion of earnings does not seem to depend on experience as for each of the top panels, and indeed for the lower panels also, the mean and the 10^{th} and 90^{th} percentile curves are approximately parallel.

With regard to returns to experience, it appears that for each class the public sector returns are slightly higher, such that the public dis-premium for $k^y = 1$ is decreased in experience while the positive public premium for $k^y = 2$ increases with experience.

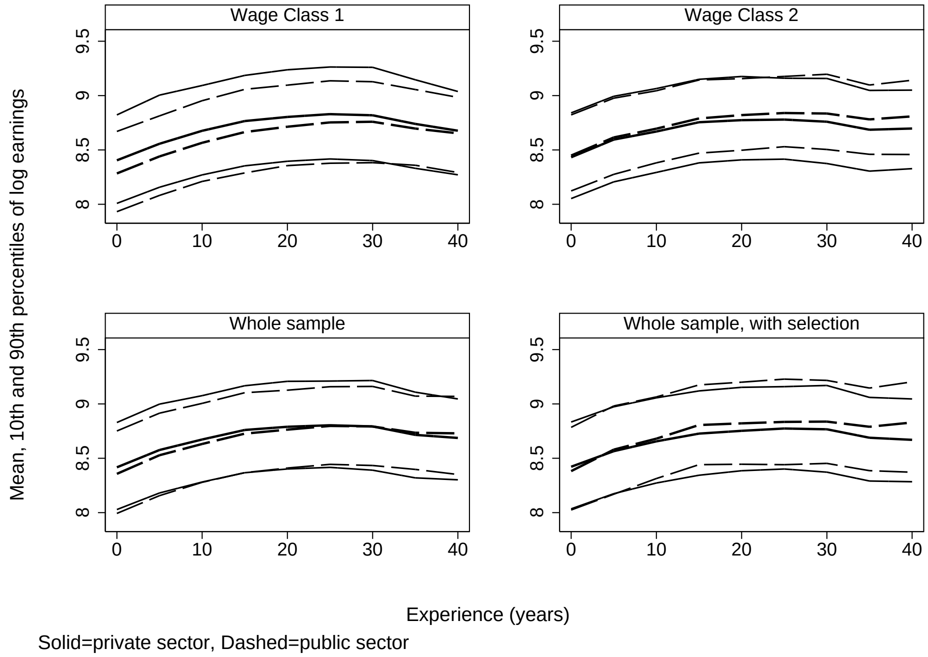


Figure 4.17: **Netherlands**: Earnings-Experience Profiles

Looking at the "whole sample" figures, the average potential public premium controlling for selection is a small negative, -2.9 log points, with a 2.1 log point reduction in the standard deviation of earnings in the public sector, again suggestive of greater pay compression in the public sector. Graphically we can see the greater public pay compression by the relative closeness of the mean and the 10^{th} and 90^{th} percentile lines for the public sector compared with the private sector, the public sector's 10^{th} (resp. 90^{th}) percentile being above (below) that of the private sector, squeezing the distribution. When we allow for selection into sector, there is a large effect on the average premium, which rises to 8.9 log points (similar to the findings of Table 4.3). Thus sorting into employment sector is clearly non-random, there is positive selection into the public sector, which affects the apparent public premium. This echoes both Hartog and Oosterbeek (1993) and Van Ophem (1993) who each find workers in the public sector have a comparative advantage in this sector.

The 1-lag auto-covariance of earnings disturbances are plotted against experience for each sector$\times$wage class in Figure 4.18, to illustrate earnings persistence differences.

For wage class 1 we see that earnings are highly persistent in both sectors, and more so for the public sector than the private, with persistence increasing slightly with experience. For wage class 2 private sector persistence is again quite high and slightly rising, but public persistence is

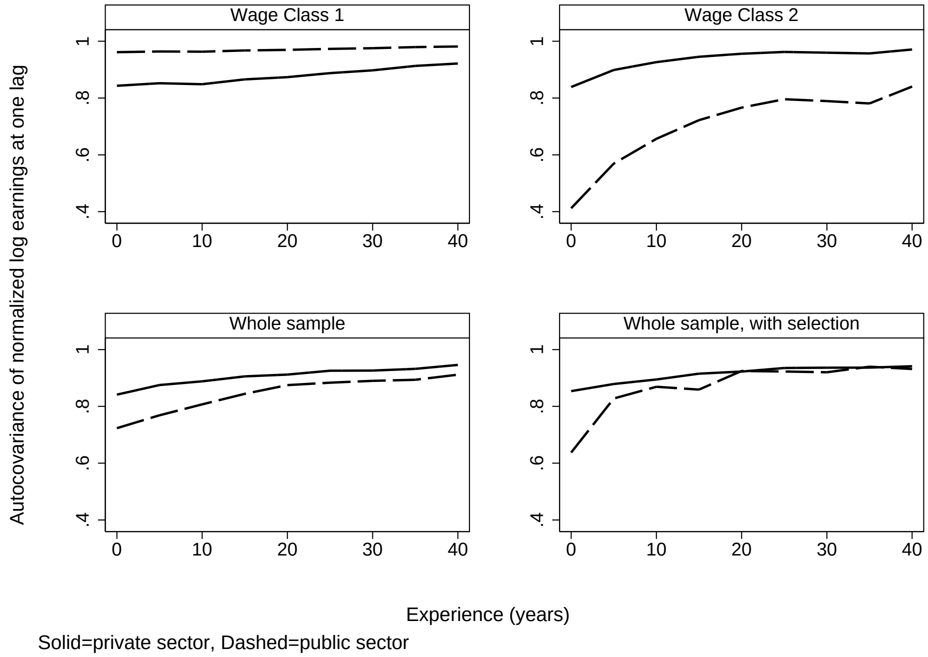


Figure 4.18: **Netherlands**: Auto-covariance of Normalised Earnings

lower – much lower initially before rising with experience to close the gap somewhat by 40 years of experience. We know from section 4.5.1 that in the Netherlands we *under*-predict earnings persistence in all but the lowest quintile at 1-lag, and this is particularly the case for the public sector for wage class 2. This is the area where the model does not fit the data very well, such that the τ_{it}^{1} for wage class 2 is under-estimated in the public sector and over-estimated in the private sector – and we see the effects of this in the figure. The observed data for this wage class suggests slightly higher persistence in the public sector but the bad fit of the model in this area reverses that finding. Looking at the bottom left, "whole sample", panel we see that the lower estimated public persistence in wage class 2 weights the overall public persistence down such that the private sector has higher persistence by this measure, over the sample. Comparing the lower two panels it appears that selection does not have an effect on this measure of private sector persistence, though it does for the public sector, with more low earnings persistence workers selecting into this sector – though we have to bear in mind the poor fit of the model in this area and interpret with caution.

The lower panel of Table 4.29 breaks down wage classes in terms of observable and unobservable characteristics. Neither wage class has a clear advantage in earnings capacity overall and it is therefore not surprising that they have very similar education and experience profiles:

each has a similar proportion with high education, wage class 2 having slightly more medium education and fewer low education, while wage class 1 has slightly more experience. With regard to the joint distribution of unobserved heterogeneity classes, all $K^m \times K^y = 6$ classes are populated, though the correlation between earnings types and mobility types is by no means perfect. The mobility type $k^m = 3$ is the most prevalent in the sample (63.6%) so it is no surprise that this mobility class dominates within each wage class, though being slightly under-represented amongst wage class 1, and thus over-represented among wage class 2.

4.5.4 *France*

Labour Market States

Starting with the sectoral composition of the mobility types, we see from the left panel ('Observed') of Table 4.31 that around half of the sample are $k^m = 3$, with approximately two-thirds of the rest of the sample type 2, the remainder being type 1. Again there are clear patterns of selection into sector for the differing mobility types, the $k^m = 1$ class having by far the highest unemployment rate, and being hugely over-represented amongst the unemployed: only 14.3% of the sample are type 1, yet 84.4% of the unemployed are of this type (henceforth referred to as the 'high unemployment' type). The $k^m = 3$ type select largely into the private sector, while the $k^m = 2$ mobility class are very much the 'public' sector types (and will be referred to as such).

With regard to their individual observed characteristics, the 'high unemployment' type clearly have much less experience than the other types (average of just 7.9 years compared to 24.2 ('public worker') and 17.0 ('private worker')), though they have a high proportion of highly educated types. This type seems to capture a mixture of younger men, who are either highly educated private sector workers just starting out or are unemployed. Unsurprisingly, the 'private worker' type have characteristics very similar to the observed characteristics of private sector workers outlined in the descriptive analysis.

The left-side, "whole sample", panel of Figure 4.19 illustrates the average predicted job loss risks for each level of experience assuming each individual in each sector. The familiar U-shape in experience is evidence for each sector, however the startling thing about this figure is that though the public sector has a lower job loss risk for the first 25 years of experience, thereafter it rises very rapidly and is approximately four times higher (at 20% risk) for 35 years of experience than in the private sector. However, when we allow for selection into sector (right-side panel) we see a dramatic reduction in the public sector job loss risk for levels of experience over 20 years, in fact it plateaus out just above zero. This is almost certainly captures something of the effect of tenure in the public sector in France in which jobs for life are guaranteed once a certain level of experience is accrued. It is also because of the human capital of public sector workers, who have greater education and experience. In addition, as Table 4.31 illustrates, the public sector draws most of its workers from mobility type $k^m = 2$, who have by far the lowest unemployment rate of all French workers.

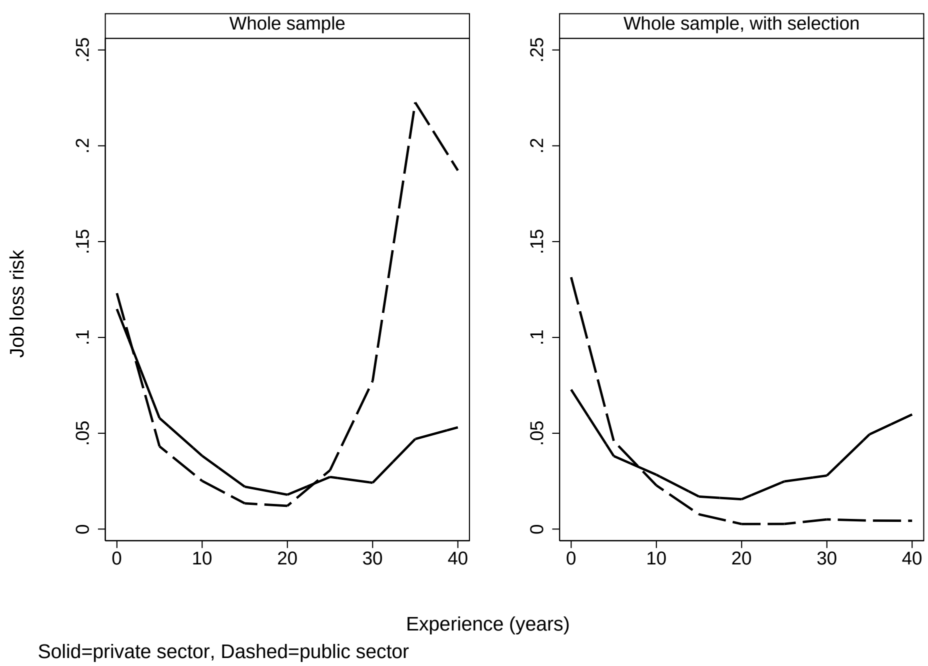


Figure 4.19: **France**: Job Loss Risk

Earnings

As is the case for Germany and the Netherlands, in France individuals are evenly divided between the wage classes with sample weights of 48.3% for $k^y = 1$ and 51.7% for $k^y = 2$. On average each class earns almost the same in the private sector (9.40 for class 1, 9.39 for class 2), but while wage class 2 has an average public premium of -1 log point, for wage class 1 the premium is $+7$ log points (see Table 4.33). Both classes have a small reduction in earnings dispersion in the public sector, the standard deviation of log earnings being 1 log point lower in the public sector for each class. Wage class 1 have greater earnings dispersion overall, 3 to 4 log points higher in each sector than is the case for wage class 2. So wage class 1 have higher average earnings in each sector but also higher earnings dispersion.

To get a visual comparison of the distributions by wage class and sector, and for the sample as a whole both with and without selection controlled for, Figure 4.20 plots mean earnings, the 10^{th} and 90^{th} percentiles of earnings, against experience for each class and for the two whole sample possibilities.

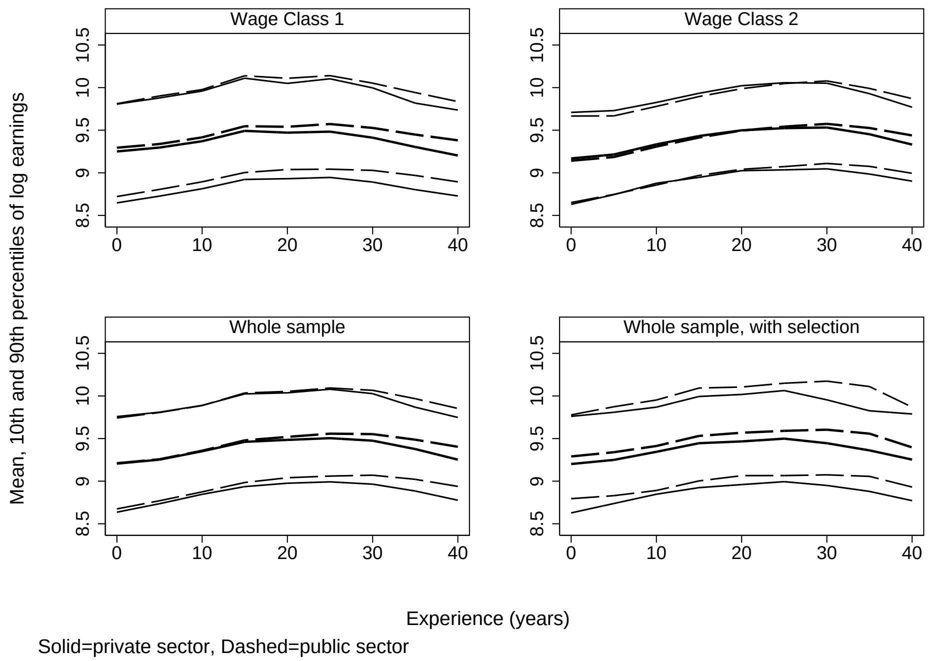


Figure 4.20: **France**: Earnings-Experience Profiles

The top two panels illustrate the finding in Table 4.33 that the public premium is higher for $k^y = 1$ types, with the mean higher in the public sector at all levels of experience, and similarly for the 10^{th} percentile. While there is some greater public pay compression for wage class 1, shown by the lower 90^{th} percentile of earnings in the public sector, even this ceases to be the case at later years of experience. In contrast for wage class 2, the various lines for the two sectors are much closer together, the public sector curves below the corresponding private sector curves initially before cutting through and opening up a premium at the mean, 10^{th} and 90^{th} percentiles of the distribution at the later years of experience.

For each class it appears that the slightly lower average dispersion of earnings in the public sector is stable throughout the working lifetime, as in all panels the within each sector the mean,

10^{th} and 90^{th} percentile lines are approximately parallel.

Looking now at the lower left "whole sample" panel, the average potential public premium controlling for selection is +3.0 log points, with a 1.5 log point reduction in the standard deviation of log earnings in the public sector – consistent with the reduction for the wage classes, and adding some limited evidence for the phenomena of greater pay compression in the public sector. Returns to experience seem to be similar initially before the public sector returns increase above the private such that a premium opens up in the mean, 10^{th} and 90^{th} percentiles for higher levels of experience. Turning to the "whole sample, with selection" panel, the average public premium in log earnings is 12.4 log points, similar to the observed data when we do not control selection (see Table 4.5). There is a public premium for all levels of experience and at each of the illustrated parts of the distribution, tending to increase in experience. This increase in premium at the mean, 10^{th} and 90^{th} percentiles, suggests positive selection into the public sector and that this selection explains around three-quarters of the raw public premium.

Now to illustrate the differences in the persistence of earnings between the sectors and wage classes, the 1-lag auto-covariance of earnings disturbances are plotted against experience in Figure 4.21.

Figure 4.21: **France**: Auto-covariance of Normalised Earnings

There is a high persistence in earnings for wage class 1 in each sector, and increasing with experience, with public sector persistence higher than private sector. For wage class 2 there is greater persistence in earnings in the public sector also, though for each sector the persistence in earnings for this class is slightly lower than for wage class 1, with a much more concave profile in experience. Again, given the even division of the sample between the wage types it is as expected that the overall picture (lower left panel) is a combination of the two classes: higher public persistence throughout, with persistence in both sectors increasing in experience, slightly more so in the private sector such that the persistence gap is closed somewhat over the work life. Comparing this picture with the lower right panel in which selection is not controlled for, illustrates that there is a small effect of selection on this measure of persistence, with the gap between the sectors reducing (public still has higher persistence), suggesting that the individuals who select into the private sector have higher earnings persistence in this sector than those who do not select into the private sector.

Table 4.32 shows the breakdown of the observable and unobservable characteristics of each wage class, in its lower panel. While potential labour market experience differs only somewhat – wage class 1 has approximately three-quarters of a year more – the education distributions are distinct: wage class 1 having simultaneously a greater proportion of high education (30.9% versus 21.1%)) and low education workers (38.5% versus 19.2%), with wage class 2 having approximately double the proportion with medium education (59.8% versus 30.6%). The greater proportion of low educated in class 1 may help to explain the greater average public premium in this wage class. This would fit with Bargain and Melly's (2008) finding that blue collar workers gain the most from public sector employment.

With respect to the joint distribution of the unobserved heterogeneity classes, each of the $K^m \times K^y = 6$ classes have at least 5% of the sample. Interestingly each wage class has a similar breakdown in terms of mobility class membership, wage class 1 having slightly more of the 'private worker' mobility class (and less of the 'high unemployment' class ($k^m = 1$)) which is somewhat surprising given that wage class 1 have a higher potential public premium. This suggests that workers select into the public sector not just because of a comparative advantage in potential earnings – 'public service motivation' may be a reason why individuals who do not necessarily earn *more* in the public sector would nevertheless chose this sector.

4.5.5 *Italy*

Labour Market States

Looking at the 'Observed' panel of Table 4.34, we see that just under half of the sample are mobility type $k^m = 1$, with the majority of the other half being $k^m = 2$ type, and only around one-sixth of the total sample falling into type $k^m = 3$. As with the other nations, there are clear patterns of selection into sector according to mobility type, though perhaps less so than in other countries. Of the $k^m = 1$ class, 89.9% are private sector employees, and have the lowest unemployment rate, only 4.1%. Mobility classes 2 and 3 are each more of a mixture, with class 3 having similar percentages (just under 30%) in each of the private and public sectors with and a slightly larger proportion (43.2%) unemployed. Class 2 has a substantially lower unemployment rate (17.6%) and more than double the proportion of workers in the public sector than the private. Thus the types can be broadly categorised as 'private worker' ($k^m = 1$), 'public worker' ($k^m = 2$) and 'high unemployment' ($k^m = 3$).

As we would expect, where a mobility class is dominated by workers selecting into a certain sector, the individual observed characteristics of that class are reflective of the sector these workers predominantly select into. The 'private worker' type, has a similar education distribution to the private sector workers described in section 4.3.5, a small proportion high education, with the majority (55.9%) medium or low educated (36.9%). Though more of the unemployed are mobility type 3, the fact that mobility types 2 (40.5%) and mobility type 1 (13.3%) do make up non-trivial proportions of the unemployed reduces the average experience in each of these types, and dramatically so, as these two types collect the unemployed who have very low experience. The mobility type 3 unemployed are disproportionately the older unemployed which inflates the average experience of this type.

With respect to job loss risk, Figure 4.22 illustrates that controlling for selection (left-side panel) for each sector the job loss risk falls almost universally with experience, rising only in the latter years. The job loss risk in the public sector is slightly lower (0.02%-points) for all levels of experience. Comparing the picture when we allow for selection into sector (right-side panel) we see that for the public sector the job loss risk remains largely unchanged, slightly lower for most levels of experience. The private sector rate is also lowered at all points such

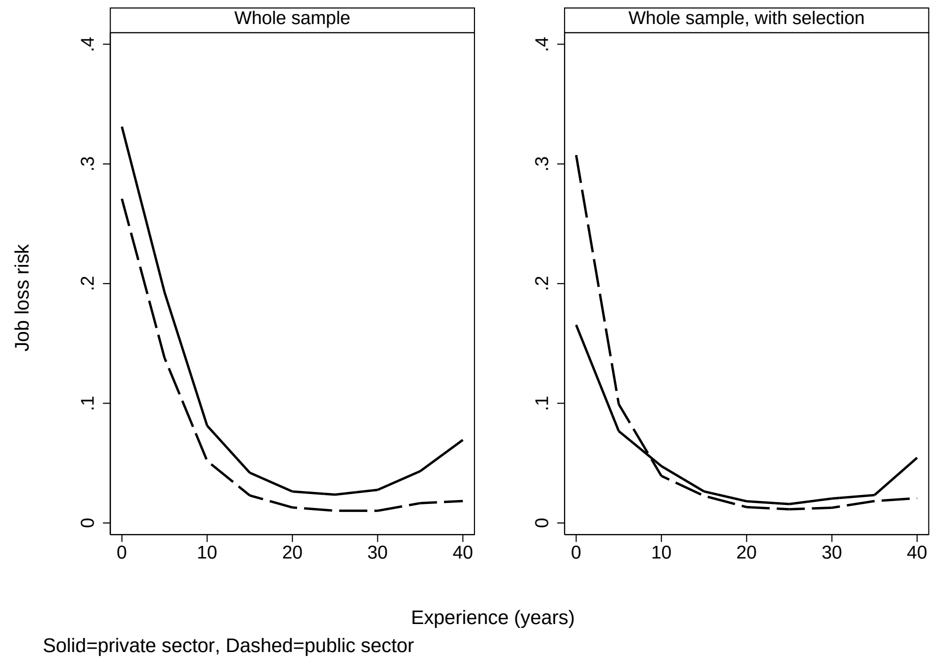


Figure 4.22: **Italy**: Job Loss Risk

that it is only just above the public sector rate for experience levels above 10 years. We know that average education and experience is higher amongst the public sector workers, hence an effect to lower the unemployment rate in the selected sample, however for the private sector, we have seen (Table 4.34) that the private sector contains many $k^m = 1$ workers who have a very low unemployment propensity, and it is selection into the private sector on this unobserved characteristic that plays a role in reducing the private sector job loss risk.

Earnings

Compared with the other nations studied thus far, Italy has a more uneven division of the sample between the wage classes with sample weights of 43.6% for $k^y = 1$ and 56.4% for $k^y = 2$. As can be seen in Table 4.36, wage class 1 has a higher average potential earnings than wage class 2 in the private sector (8.01 versus 7.86) and the public sector (8.03 versus 7.88). Wage class 1 also has higher dispersion of earnings in each sector as measured by the standard deviation of log earnings: 0.34 (class 1) versus 0.26 (class 2) in the private sector with corresponding figures of 0.33 and 0.24 for the public sector. In terms of public premia, wage class 1 types (the 'high earners') earn a 2 log point higher average in the public sector compared with the private sector, and the same is true of wage class 2.

These differences are visually confirmed in the top two panels of Figure 4.23, the small average public premium for each class driven by higher average starting wages in the public sector.

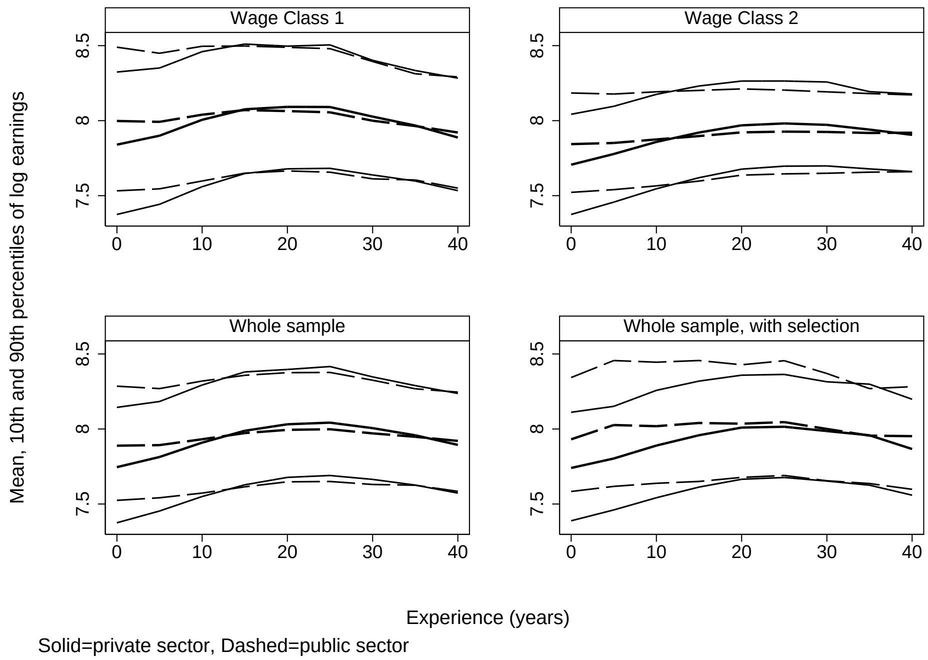


Figure 4.23: **Italy**: Earnings-Experience Profiles

We can see that for each sector, wage class 2 has a much more compressed earnings distribution than wage class 1, the 10^{th} and 90^{th} percentiles of earnings much closer to the mean in the right side panel. The top two panels also show how the wage class 1 mean and the percentiles shown are higher than the corresponding curves for wage class 2, irrespective of sector.

It is striking that the returns to experience appear substantially higher in the private sector than the public sector in Italy, for both wage classes, especially in the early years of the working lifetime. For each class, between 20 and 30 years of experience these higher private sector returns taper off but not before they have eliminated the initial public premium. Again, the dispersion of earnings in each class does not seem to be a function of experience, each panel of Figure 4.23 showing that each set of lines for the mean, 10^{th} and 90^{th} percentiles of earnings maintain their positions relative to each other as experience increases.

The average potential public premium across the whole sample, controlling for selection, (lower left panel), is a modest 2.1 log points with the standard deviation of earnings reduced by 1.4 log points, consistent with the findings for each wage class. However, non-random sorting into wage classes is very important as when we allow for selection into sector, the average public premium rises to 10.4 log points (which is slightly higher but consistent with the finding in Table 4.7).

Using the the 1-lag auto-covariance of earnings disturbances as a measure of earnings persistence, we look at difference between the classes and sectors and how they alter with experience in Figure 4.24.

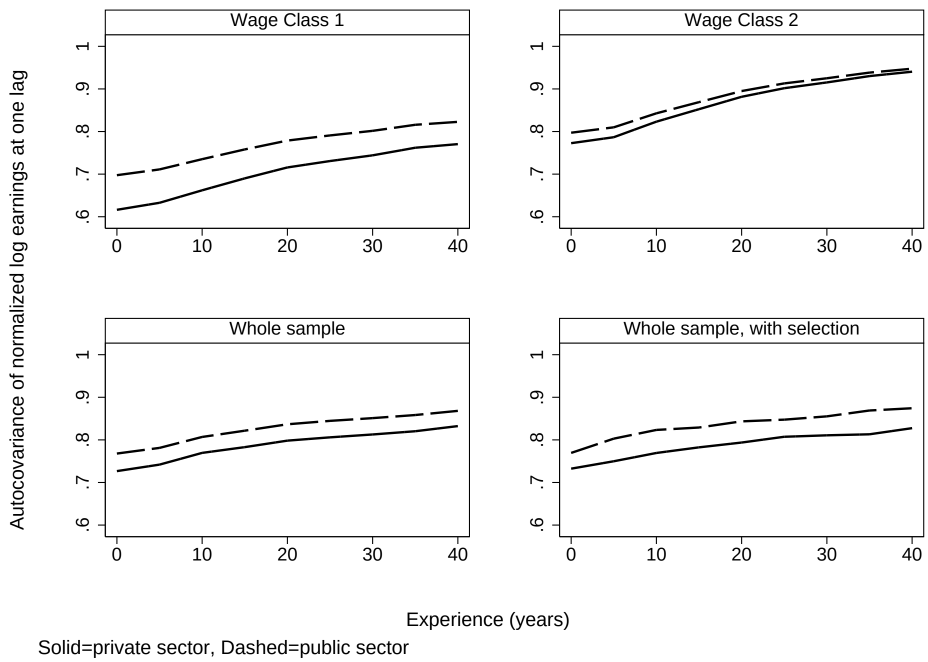


Figure 4.24: **Italy**: Auto-covariance of Normalised Earnings

As we are accustomed to finding, the earnings persistence is greater in the public sector than the private sector for each wage class, and for each class and sector, persistence in earnings is increasing steadily in experience. The 'higher earners' ($k^y = 1$) exhibit lower earnings persistence in each sector than the lower earnings class, and the gap between persistence in the public and private sector is greater for these 'higher earners' and does not close over the lifetime. For the lower earning class, the difference in persistence between the public and private sectors is small initially and even closes slightly with experience to be almost eliminated for very high level of experience.

Given the sample weights in favour of wage class 2, the "whole sample" picture is closer to the wage class 2 picture. The gap between the sectors in terms of persistence remains stable over the lifetime, the private sector persistence only slightly closer to the higher public sector level for 40 years experience compared than it is for zero experience. Comparison of the "whole sample" and the "whole sample, with selection" panels suggests that selection does not have a large impact on this particular measure of earnings persistence.

The lower panel of Table 4.35 shows the breakdown of observable and unobservable characteristics for each wage class. Unsurprisingly the 'higher earner' wage class have a substantially greater proportion of highly educated workers (13.9% versus 5.3%) and a lower proportion of low educated workers (32.7% versus 44.6%), whilst also having more than 5 years more potential labour market experience on average (20.6 years versus 15.1). The joint distribution of unobserved heterogeneity classes shows that each of the $K^m \times K^y = 6$ classes are populated by at least 5% of the sample. Each wage class has a similar proportion of $k^m = 1$ types, differing mainly in their proportion of mobility types 2 and 3. The 'lower earner' wage class has a greater proportion of $k^m = 2$ types who are the 'public worker' types – perhaps collecting the lower earning public sector workers – while the 'higher earner' wage class collects a greater proportion of the 'high unemployment' mobility type. Though the dimensions of unobserved heterogeneity are not independent, the correlation is not straightforward. Again this suggests that potential earnings premia are not the only motivation behind selection into the public sector.

4.5.6 *Spain*

Labour Market States

The left panel of Table 4.37 shows the sectoral×mobility class breakdown for the observed sample data, revealing that just under 60% of the sample sort into mobility class 3, with the remainder evenly split between classes 1 and 2. The vast majority (93.4%) of class 1 are public sector attached workers, while not quite so dramatic a sorting happens for class 3, 87.8% of whom select into the private sector. The $k^m = 2$ mobility class is more of a mixture, with by far the highest unemployment rate of the three classes, and the majority of the unemployed being drawn from this class. (Hence the mobility classes will be referred to as 1 'public worker', 2 'high unemployment' and 3 'private worker').

With respect to the observable human capital characteristics of the classes, the 'private worker' type have an education breakdown that almost perfectly mirrors the private sector breakdown from section 4.3.6, and the 'public worker' type similarly has an education profile –

with more than double the proportion of high educated than the 'private type' – which is very similar to the public sector breakdown observed in the descriptive analysis. Potential experience levels for each mobility class similarly reflect the experience levels in the sector into which each type predominantly selects.

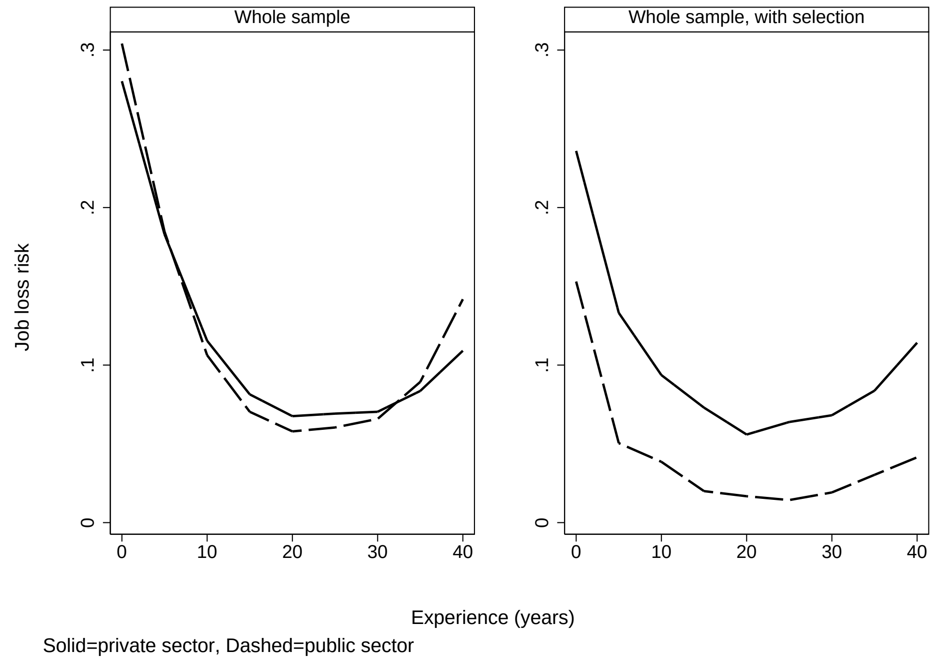


Figure 4.25: **Spain**: Job Loss Risk

In terms of job loss risk, when controlling for selection into sector we can see from the left-panel of Figure 4.25 that job loss risk traces a U-shape in experience, with very little difference between the predicted risk in either sector. When we allow selection into sectors however, both the public and the private sector job loss risks reduce at almost all levels of experience, with the largest effect being on the public sector risk. From 10 years experience onwards the workers who selected into the public sector have a lower job loss risk by around 0.05% points. From the descriptive analysis we know that a much greater proportion of the public sector workers in Spain have high education compared with the private sector (51.0% versus 28.7%) and have more experience on average (23.0 years versus 21.6) and this clearly has an effect on the differing job loss risks in the selected sample. Moreover, as discussed above the public sector also has a high proportion of $k^m = 1$ mobility type workers who have very low rates of unemployment (3.23%), reinforcing the effects on job loss risk of the observed human captial.

Earnings

For Spain, like Italy, the sample division between the wage classes is quite uneven: 43.3% are $k^y = 1$, 57.7% are $k^y = 2$. In respect of earnings levels, the smaller wage class 1 are the 'higher earners', see Table 4.39. Wage class 1 types earn more than wage class 2 in both the private sector (12.33 versus 12.16) and the public sector (12.42 versus 12.29). We can see that wage class 2 earn a greater public premium (+13 log points against +9) thus the earnings gap between the types reduces in the public sector. For both classes, the higher public earnings are accompanied by a lower spread of earnings in the public sector, the standard deviation of log earnings being 4 log points (resp. 2 log points) lower in the public sector for wage class 1 (resp. wage class 2).

Figure 4.26 plots the mean, 10^{th} and 90^{th} percentiles of earnings in each sector against experience: in the upper two panels separately by wage class, and in the lower panels plots for the "whole sample" i.e. assuming each individual is in each sector, and for the "whole sample, with selection" in which we only include predicted earnings in the private (resp. public) sector for individuals observed in the first period of the real data in the private (resp. public) sector.

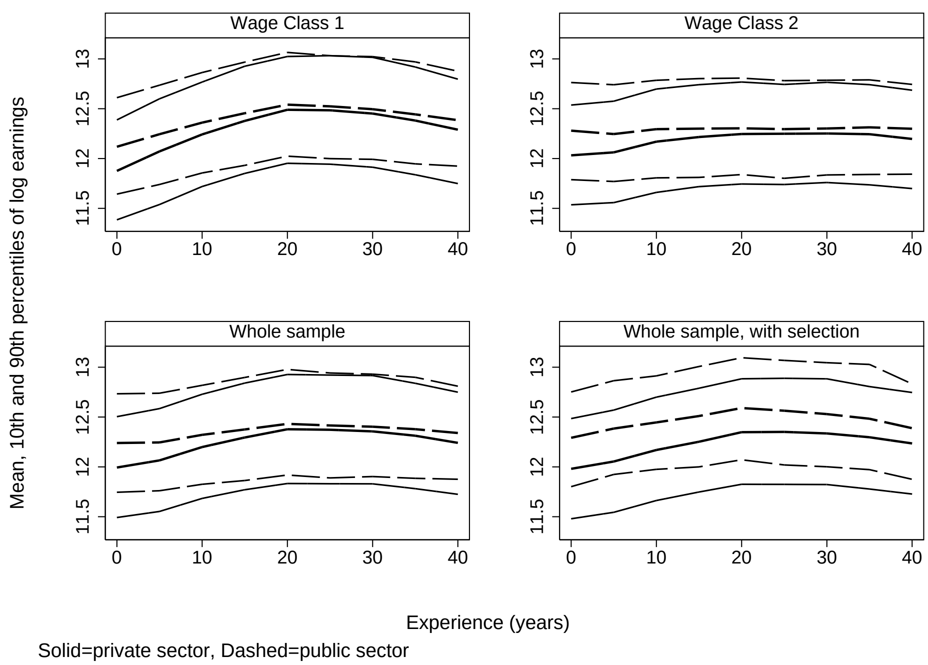


Figure 4.26: **Spain**: Earnings-Experience Profiles

The top two panels illustrate the findings of Table 4.39. We can see that at each of the included points of the distribution (mean, 10^{th}, 90^{th} percentile) the wage class 1 'high earners' have higher earnings than wage class 2 for almost all experience levels, regardless of the sector. Moreover, in each sector the wage class 1 returns to experience are notably greater than those of wage class 2, who have much flatter trajectories, particularly so for the public sector. Also in terms of returns to experience, within each class they are notably higher in the private sector, at least for the 25 to 30 years of work experience. In terms of the public premium for each wage class, we can see that the premium is in evidence not just at the mean but at the 10^{th} and 90^{th} percentiles of earnings also – so public sector earnings are higher across the board for both wage classes. As noted, the dispersion of earnings in the public sector is lower for each class than it is in the private sector, and the figure confirms that this is because of the higher premiums at the 10^{th} percentile and the mean as compared with the 90^{th} percentile, thus the public lines are slightly closer because of this relative compression at the top of the distribution. It is notable that experience does not seem to alter the extent of earnings dispersion, as for each panel the lines, within a sector are approximately parallel.

Across the "whole sample", the average potential public premium in earnings is +11.1 log points, with a 3.4 log point reduction in the standard deviation of log earnings for the public sector, suggesting greater pay compression in the public sector. Graphically the figure illustrates that the public premium in earnings is in evidence not only at the mean but at the 10^{th} and 90^{th} percentiles of earnings also – as we would expect given this finding in each class – and the slightly greater compression of earnings in the public sector is reflected in these lines being closer for the public sector than the corresponding curves for the private sector. Similarly we see again that the returns to experience are greater in the private sector for at least the first 25 years of experience. Allowing for self selection into each sector, lower right panel, the public premium is much increased at every illustrated point of the distribution – averaging 27.1 log points (which fits well with the finding in the descriptive analysis of Table 4.9). Clearly non-random sorting into employment sector plays a large part in explaining the public premium in earnings, though the left panel suggests that even controlling for positive selection into the public sector, there is still a substantial public premium. The findings echo those of Lassibille, in there existing a public premium even when selection is controlled but that returns to human capital are lower in the public sector.

Again, to illustrate the differences in the persistence of earnings between the sectors and wage classes, we can plot the 1-lag auto-covariance of earnings disturbances against experience; this is done in Figure 4.27.

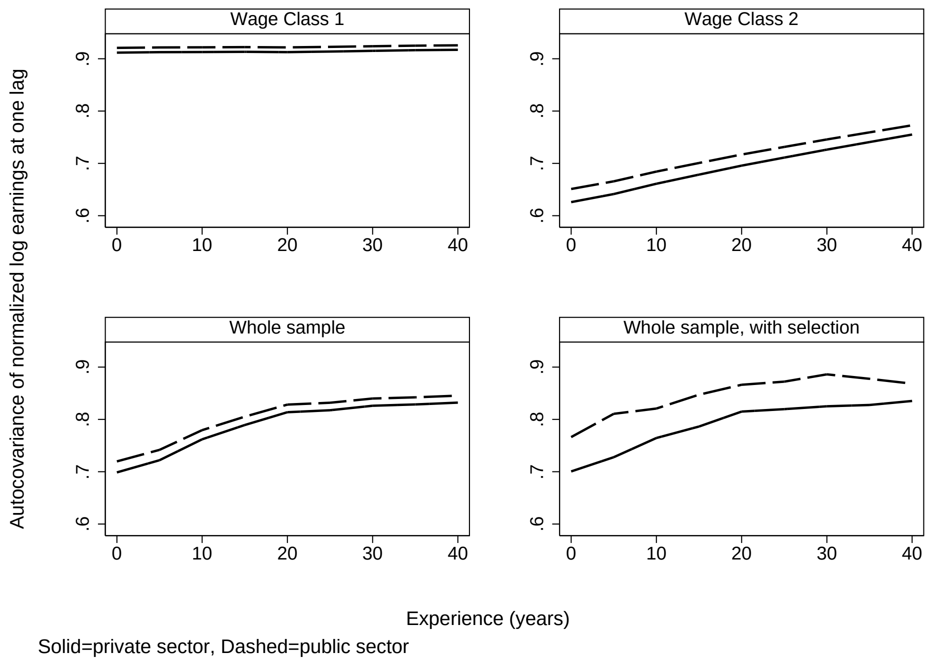


Figure 4.27: **Spain**: Auto-covariance of Normalised Earnings

We can see that for each wage class, the estimated persistence of earnings is higher in the public sector than in the private sector, but beyond this the similarity between the classes is somewhat limited. For wage class 1, the 'higher earners', there is extremely high earnings persistence for both sectors, public slightly above the private sector, and that this does not alter with experience – in each sector the persistence is almost constant across all levels of experience. In contrast, for wage class 2 the initial earnings persistence at zero experience is for each sector approximately just two-thirds the level for wage class 1, but increases steadily in experience such that the gap is approximately halved for each sector *vis-a-vis* corresponding sector for wage class 1 by the end of the working lifetime. The overall picture, lower left panel, is weighted more heavily towards the more prevalent wage class 2 types, and as starts with each sector having persistence quite close to that of wage class 2 though higher due to the influence of the high wage class 1 persistence. As with each class, the public sector has slightly higher persistence, and for each sector, persistence increases with experience at a similar rate such that the differential in persistence between the sectors is maintained. Comparison of the lower left and lower right panels suggests that selection into sector matters for this measure of persistence, in that while the private sector curve is barely altered, it seems that those with higher public sector earnings persistence select into that sector, shifting the public curve upwards and increasing the gap between the sectors.

In the lower panel of Table 4.38 we have the education and experience breakdown for each of the wage classes, as well as the joint distribution of unobserved heterogeneity classes. Unsurprisingly given the higher earnings of wage class 1 in each sector, we find that $k^y = 1$ types have much higher education on average than $k^y = 2$ types (40.7% of $k^y = 1$ are high educated, 26.0% medium, compared with 28.1% high and 20.7% medium) and around 4 more years of potential labour market experience (20.3 years versus 16.4).

In terms of unobserved heterogeneity, we find that only five of the $K^m \times K^y = 6$ classes are populated: there is negligible weight in the $k^y = 1, k^m = 2$ class. This is a combination of the 'high earner' wage class with the 'high unemployment' mobility class, so it is clearly no accident that this combination is not found in the sample data. Given the large public premium in each wage class, it is not surprising either to find a strong correlation between the 'high earner' wage class and the 'public worker' mobility class i.e. $k^y = 1, k^m = 1$.

4.5.7 *Portugal*

Labour Market States

Looking at the 'Observed' data panel of Table 4.40 we see that 62.4% of the sample are mobility class 2, with the remainder split unevenly between class 1 (25.5%) and class 3 (12.1%). The most notable sector selection pattern across the mobility types, is that $k^m = 2$ types are almost exclusively in the private sector (94.5%) with the remainder unemployed, with no public sector workers of this type (we will refer to this type as 'private worker'). The public sector draws its workers almost completely from mobility class 1 ('public worker'), though within this class there are also private sector workers and an unemployment rate of 8.2%. The smallest mobility class, $k^m = 3$, has the highest unemployment rate (23.8%) and is over-represented amongst the unemployed (hence 'high unemployment' type).

In terms of observable characteristics, the 'private worker' class, unsurprisingly closely mirror the education breakdown of the private sector, with the vast majority (81.8%) low educated, and the majority of the remainder only medium educated. Similarly the 'public worker' class almost identically reflects the education composition of the public sector with 16.0% high, 20.8% medium and 63.2% low educated. The 'high unemployment' mobility class attracts the more experienced private sector workers and the more experienced unemployed individuals.

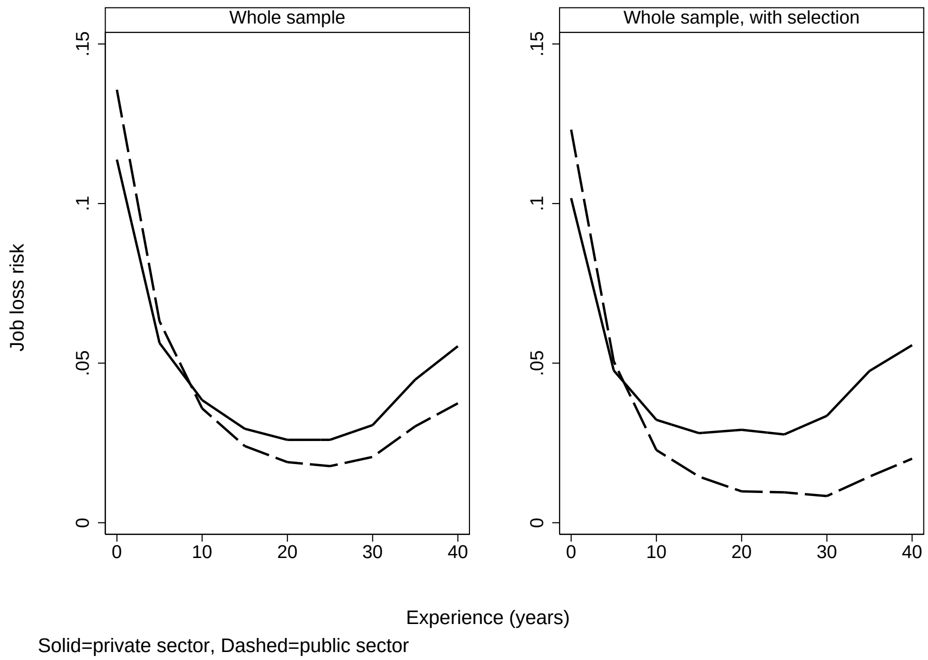


Figure 4.28: **Portugal**: Job Loss Risk

As Figure 4.28 illustrates, we find the usual U-shape in experience traced by both the public and the private job loss risk when we control for selection, in the "whole sample" left-side panel. For all but the early years of experience, the public sector job loss risk is consistently lower than the private sector risk, and is approximately three-quarters of the level of risk of the private sector at around 2.5% job loss risk from 15 to 35 years of experience. When we allow for selection into sector, the effect on the private sector job loss risk is minimal. With regard to the public sector however the reduction in risk is substantial, with the risk approximately halving for years of experience above 15-20. Again we know that the public sector has workers with higher human capital levels (15.6% are high education in the public sector, compared with 6.0% of the private sector) and this appears to drive the selection effect.

Earnings

The Portugal sample is split unevenly between the two wage classes, with sample weights of 45.7% for $k^y = 1$ and 52.3% for $k^y = 2$. As we can see from Table 4.42, wage class 2 has slightly higher average potential earnings in the public sector (11.84 versus 11.81 for wage class 1), however in the private sector the gap is much larger in the wage class 2 types' favour: 11.79 versus 11.70 for wage class 1. Thus wage class 1 are 'low earners' type and particularly low private earners. The public premium in average potential earnings for wage class 1 is +11 log points, while for wage class 2 it is a more modest +5 log points. Consistent with the findings of the descriptive analysis of subsection 4.3.7, the earnings dispersion is actually *higher* in the public sector than the private sector, and this is the case for each wage class: wage class 1 increasing the standard deviation of log earnings from 0.38 log points (private sector) to 0.45 (public). The 'high earner' wage class 2 types also have higher dispersion than wage class 1 in each sector: 0.45 log points (private), 0.53 (public).

These patterns are illustrated in Figure 4.29 which plots mean earnings, the 10^{th} and 90^{th} percentiles of earnings, against experience for each wage class and sector separately and for the "whole sample" and the "whole sample, with selection".

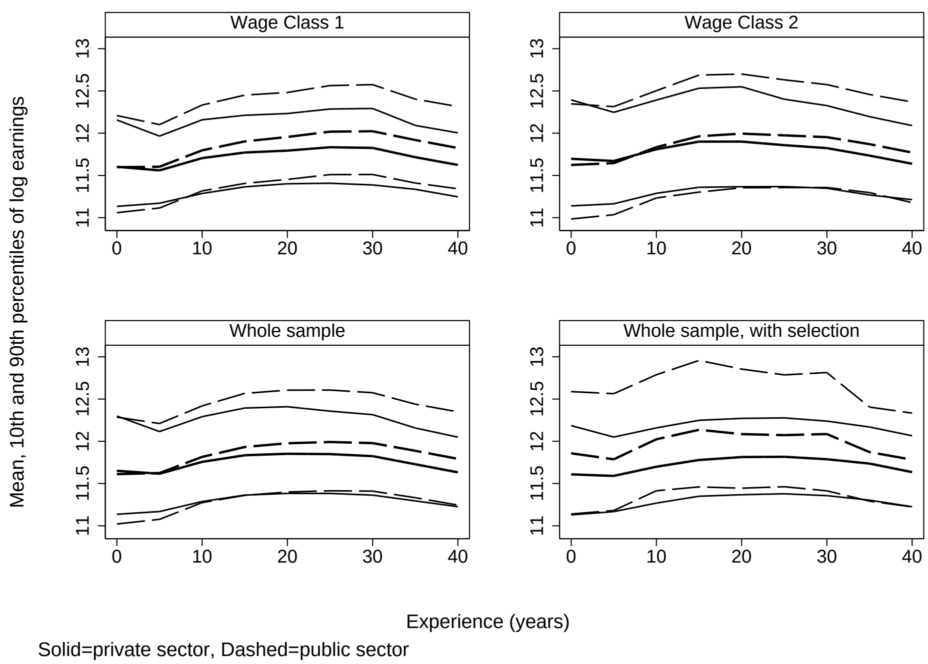


Figure 4.29: **Portugal**: Earnings-Experience Profiles

We can see the greater dispersion of earnings in the public sector for each wage class through the greater distances between the mean, 10^{th} and 90^{th} percentiles of earnings, especially for wage class 2. Moreover we can see how wage class 1 have lower earnings than wage class 2 in the private sector, and how this results in the greater public premium for wage class 1. It is also clear that while for wage class 1, the public premium is evident over all levels of experience in the mean and at the 90^{th} percentile, it is not initially in the 10^{th} percentile. Similarly for wage class 2, the premium at the 90^{th} percentile is across all experience levels but for the mean it is only after 10 years of experience that the premium is evident and at the 10^{th} percentile the public sector earnings are below the private sector throughout. For each wage class the returns to experience are greater in the public sector for at least the first half of a working lifetime, before tapering off and being comparable to private returns in the second half. The dispersion of earnings does not appear to be a function of experience as we see that in each panel for each sector the lines for the mean, 10^{th} and 90^{th} percentiles are approximately parallel.

The average premium in potential earnings across the whole sample, controlling for selection, is 7.4 log points (illustrated lower left panel) with the standard deviation of log earnings increasing in the public sector by 7.6 log points, consistent with the findings for each wage class and the sample weights. Allowing selection into sector, the premium is vastly increased to 31.2 log points (which is slightly higher but still consistent with the premium found when not controlling selection in descriptive analysis of Table 4.11). Clearly the selection into sector is very important in explaining the raw public premium in earnings, with approximately three-quarters of the average premium explained by selection of workers into the public according to positive characteristics.

Again using the 1-lag auto-covariance of earnings disturbances as an index of earnings persistence, we examine how the classes and sectors differ over the working life time in Figure 4.30.

For each wage class we find that earnings are more persistent in the public sector, with the difference between public and private sector persistence narrowing somewhat over the course of the work lifetime. The classes differ markedly in their persistence profiles, wage class 1 having substantially lower persistence than wage class 2 (the higher private sector earners) at the start of the lifetime, but with a much steeper increase in experience such that after 10-15 years of experience wage class 1 persistence is higher for each sector than wage class 2.

As the sample is weighted more towards wage class 2, the aggregate picture, lower left panel, is more like the wage class 2 profile with less of a steep increase in persistence with experience. Selection effects seem to matter to a certain extent, as the right panel shows when we allow

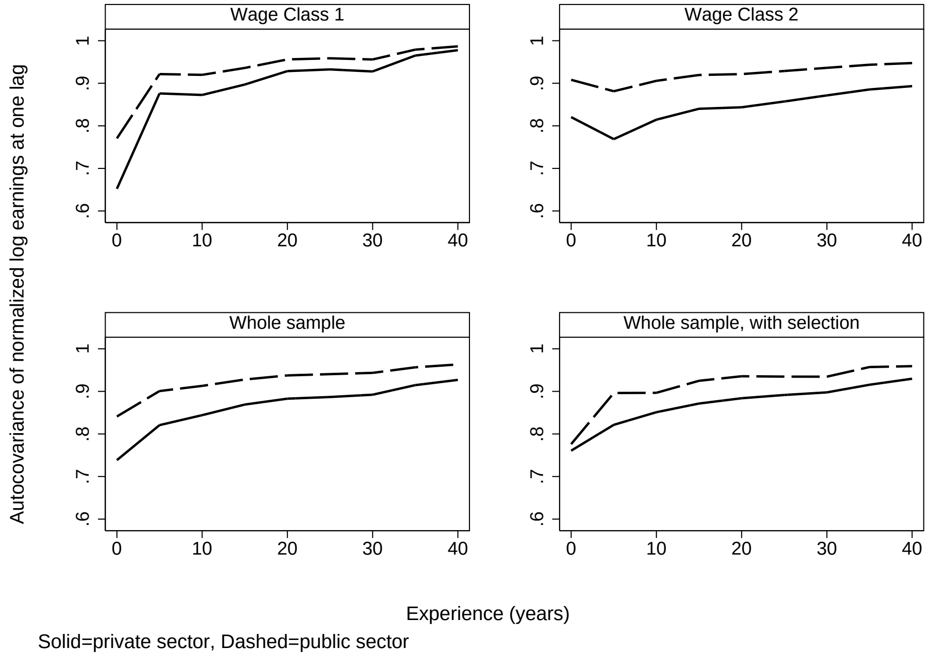


Figure 4.30: **Portugal**: Auto-covariance of Normalised Earnings

selection it appears that those with slightly higher private sector persistence select into this sector, while those with slightly lower public sector persistence select into that sector.

Finally, the lower panel of Table 4.41 illustrates the education and experience breakdown for the wage classes, as well as the joint distribution of the unobserved heterogeneity classes. We find that a greater proportion of the 'higher earners' ($k^y = 2$) are high educated (9.3% compared with 6.0% for wage class 1) and have on average 2 years more experience than the wage class 1 type (18.4 years versus 16.4). Almost all of the $K^m \times K^y = 6$ classes contain a non-negligible proportion of the sample, the exception being the combination $k^y = 1, k^m = 3$: which is a combination of the high public premium wage class and the 'high unemployment' mobility class, so perhaps not a surprise that it is not a combination found very much in the data. Though these two dimensions of heterogeneity are not independent, the correlations are not straightforward, with for example as many of the lower private earnings types as those with higher private earnings amongst the mobility class ($k^m = 2$) which is mainly private sector workers.

4.5.8 *Summary of Results*

The results from the model estimation, controlling for non-random selection of workers into employment sectors, can be summarized as follows:

- **Public Premium**: There is a small average public pay premium in France (+3.0 log points) and Italy (+2.1), and larger premia in Spain (11.1 log points) and Portugal (7.4). The public premium is small and negative for Germany (−0.8 log points) and the Netherlands (−2.9).

- **Returns to Experience**: The returns to experience are greater in the public sector than the private sector for Germany, the Netherlands, France and Portugal. As such the average predicted public premium increases with experience – in Germany the negative premium turns positive after 15 years of experience, in the Netherlands the negative premium reduces to zero by 40 years of experience. Conversely, in Italy and Spain the private sector has higher returns to experience, thus the public pay premium falls with experience, turning negative after 15 years of experience in Italy, but remaining positive albeit reduced for all levels of experience in Spain.

- **Earnings Persistence**: For each country (except the Netherlands), earnings persistence is greater in the public sector than in the private sector, and is increasing with experience for each sector. For the Netherlands, persistence increases with experience in each sector but is estimated to be higher in the private sector[47].

- **Earnings Dispersion**: The standard deviation of log earnings is lower in the public sector for Germany (by 1.2 log points), the Netherlands (1.0), France (1.5), Italy (1.4) and Spain (3.4). For Portugal the standard deviation of log earnings is *higher* in the public sector by 7.6 log points.

- **Job Loss Rates**: Predicted job loss rates are lower in the public sector, for for the greater part of the experience range if not all, in the Netherlands, Italy, Spain and Portugal. In France the predicted public sector job loss rate is lower for the first 25 years of experience,

[47] Though with the caveat that the fit of earnings persistence is not as good in the Netherlands as elsewhere.

then higher thereafter, while in Germany the predicted public job loss rate is higher for all levels of experience.

There is however a very marked effect of selection on these findings and it appears in each country that there is selection into the public sector on characteristics that increase earnings. For all countries, when we allow non-random selection into sectors we find a positive average premium in earnings, ranging from 4.7 log points (Germany) to 31.2 log points (Portugal). For Spain, Italy and the Netherlands, selection also increases the persistence of earnings in the public sector relative to the private sector, while in the other countries selection does not affect persistence of earnings. Job loss rates are also differentially affected by selection for all countries: in each case the private sector job loss rate is largely unchanged by selection whereas the public sector predicted job loss rate is substantially lowered for the selected sample. In sum, selection into the public sector is clearly important in explaining the higher level of earnings, greater persistence of earnings and lower job loss risk in the public sector.

4.6 The Public Pay Gap: Earnings and Lifetime Values

As alluded to above, in this section we develop a more systematic analysis of selection into the public sector and the differences between the sectors in terms of earnings flows and lifetime values. The first thing we do is construct the lifetime values and then consider the differences across sectors. We then engage in some counterfactual simulations in which individuals are simulated for a 'lifetime' in each sector, and consider the differences under such counterfactual assumptions. Before presenting the results for each country, we first establish the mechanics of the lifetime values construction.

4.6.1 Construction of Lifetime Values

The notion of lifetime value that we shall use here is simply the present discounted sum of future income flows, which is the relevant measure when individuals are either risk-neutral or can perfectly insure. Using our estimated coefficients for earnings distributions, and earnings and job mobility, we can carry out simulations of employment and earnings trajectories for the individuals in our sample until retirement age which we assume to happen at a level of experience denoted T_R. We assume that after retirement a given individual enjoys a present discounted sum of future earnings stream of V_R (defined below). Given these assumptions, the lifetime value at experience level t of an individual's simulated future earnings trajectory $\mathbf{y}_{s\geq t}$ is written as:

$$V_t\left(\mathbf{y}_{s\geq t}\right)=\sum_{s=t}^{T_R}\beta^{s-t}\cdot\exp\left(y_s\right)+\beta^{T_R-t}\cdot V_R, \tag{4.15}$$

where $\beta\in(0,1)$ is a discount factor and $\exp\left(y_t\right)$ is the earnings flow that the individual receives at experience level t; y_t designates log earnings.

As raised in subsection 4.4.1, the fact that we do not observe earnings when an individual is unemployed means that for the purposes of constructing a lifetime value – allowing for movement into and out of unemployment, and at different rates according to the sector – we need to make some assumptions regarding replacement rates (δ) for unemployment earnings. We use figures from the OECD on gross replacement rates, which themselves are averages over a number of different demographic/family structure categories, and replace an individual's earnings in unemployment at time t with δ times their predicted private sector earnings given their characteristics and experience at time t, where the δ are the country-specific rates[48]. At each level of experience t, current log earnings y_t are conditional on the individual's characteristics and labour market state, as set out in the statistical model of section 4.4 and more specifically spelled out in Appendix C.2.

For all countries we set the discount factor to $\beta=0.95$ *per annum*. The value of retirement, V_R, is defined as $V_R=\frac{1-\beta^{20}}{1-\beta}\times RR\times\exp\left(y_{T_R-1}\right)$, where RR designates the replacement ratio. Thus we assume that after retirement, individuals receive a constant flow of income equal to RR times their last earnings in employment and discount this flow over a residual life expectancy of twenty years. We calibrate the value of RR to 0.40 and the experience level at retirement to 45 years. While these values will be a more accurate reflection of reality in some countries more than others, again in the interest of having a common framework for all countries, we impose these common parameters.

One caveat that must be flagged at this point, is that in conducting this lifetime simulation exercise, we have to assume that, in each country, the economic environment is stationary. We

[48] These rates are: Germany 25%, Netherlands 50%, France 40%, Italy 25%, Spain 35%, Portugal 40%.

assume that agents anticipate getting older and experiencing wage mobility and job mobility given their current wage and job status, but that they do not anticipate any changes in the model parameters over the remainder of their working lives. For this to be a reasonable assumption it requires our sample time period in each case to be a fairly representative of the average state of the business cycle. As was demonstrated in section 4.3, it is more or less the case in each country that the economic environment is reasonably stable, though with some country specific fluctuations. Whilst it is unlikely that the economic environment does remain stable throughout their working life, the assumption of stability is the best guess individuals may make when forming expectations of their lifetime earnings stream.

Within each country, in order to investigate the role of differences in earnings mobility between the sectors in generating public premia in log earnings flows and lifetime values, we run a series of counterfactual simulations in which we constrain the probability of moving between sectors or into unemployment to be zero. That is, we assign individuals to a 'job for life' in each sector and simulate their earnings trajectories. In this case, the only sources of differences in lifetime values are therefore difference in cross sectional earnings and differences in earnings mobility across the sectors. The first part of the analysis for each country therefore looks at *the role of earnings mobility*.

In each of the sections, we look at the public premium both in log earnings flows and in log lifetime values at each percentile of their distributions. The public premium in this case is defined as the difference between the log earnings (resp. log lifetime value) in the public sector and the private sector at each percentile point in the distribution.

For each section and country we have two pictures: one is the "whole sample" and the other is the "whole sample, with selection". The left panel, "whole sample", refers to the case where we simulate a separate lifetime in each job sector for each individual in the sample, thus we get a their *potential* earnings flow and lifetime value in each sector, with every individual in the sample included in each sector. In this way we control for selection effects as we have a 'copy' of each individual in each sector. We then calculate the difference in the public and private sector distribution of log earnings and log lifetime values at each percentile. The "whole sample, with selection" panel refers to the situation in which we categorise an individual to be a 'private sector worker' if in the real data we first observe them in the private sector, and those we first observe in the public sector we categorise to be a 'public sector worker'. We then use only the simulated working lifetime in the private (resp. public) sector for those we have assigned as private (resp. public) sector workers. Thus we allow for selection into sector, and then compare the distributions, calculating the public premium in log earnings and log lifetime values in the same way as for the "whole sample" case. Therefore the "whole sample, with selection" picture plots the "raw" (unconditional) difference between the distribution in each sector.

Following this, we repeat the analysis, but this time simulating job as well as earnings mobility. We do this by adopting an alternative definition of sector-specific lifetime values: we again simulate one 'lifetime' in each sector for each individual, but rather than imposing that the individual remain in that initial job sector for the *entire* working life, we impose only that the individual be in that sector for the *initial* period. We then simulate earnings trajectories allowing the individual to move between sectors, according to their predicted transition probabilities. Thus we analyse *the role of job mobility*. The "whole sample" and "whole sample, with selection" cases here are constructed exactly as described above for the 'job for life' scenario i.e. when allowing selection, it is the sector in which the individual was first observed that defines whether they are a 'private (resp. public) sector worker'. The difference from the 'job for life' case being that a 'private sector worker' included in the private distribution when calculating the public premium is only constrained to be in the private sector for the initial period, after that they may move in and out of each labour market state.

4.6.2 Germany

The Role of Earnings Mobility

Figure 4.31 shows the public premium both in terms of log earnings flows – calculated for first period earnings – and log lifetime values by percentiles in their respective distributions, given the 'job for life' assumption[49]. Looking at the "whole sample" picture we can see that in terms of predicted earnings, the public premium is zero at the lowest percentiles and then falls steadily as we move up the distribution such that there is a negative premium of almost 5 log points at the top of the distribution. The pattern of the public premium falling as we move up the distribution is consistent with there being greater pay compression in the public sector. The

[49]The extremes of each distribution are trimmed to focus on the main story of each figure.

picture for lifetime values is very similar, the premium tracing almost exactly the same shape as we move up the distribution, though with the whole curve shifted up by around 5 log points. As such, the initial public premium in log lifetime values is approximately 5 log points but then steadily falls to be almost zero at the very top of the distribution. The fact that there is a positive public premium in lifetime values across the whole range, suggests that the negative public premium in initial cross-sectional earnings is offset by the earnings mobility differences: as shown in section 4.5.2, the returns to experience are higher in the public sector, such that the initial negative public premium in earnings falls and then reverses for the second part of a working life time. This is taken into account in the lifetime values and this seems to be the driving factor behind the small positive public premium in lifetime values.

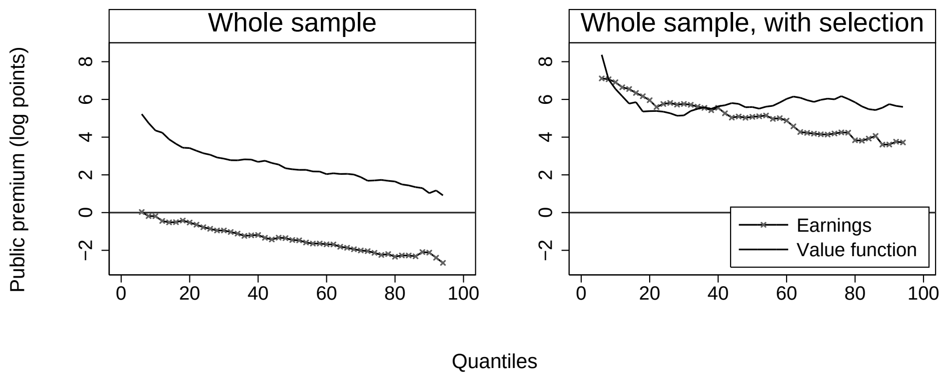


Figure 4.31: **Germany**: The Public Gap, 'Jobs for Life'

Looking at the "whole sample, with selection" panel, we see that when we allow selection, the public premium in earnings shifts up considerably so that there is a positive public premium at all points of the distribution. This suggests that selection is important and that there is positive selection into the public sector, individuals who have greater potential earnings and employability select into the public sector. As commented on above, the premium falls as we move up the distribution, consistent with the well observed phenomena of pay compression in the public sector, though it still averages around +5 log points (consistent with the descriptive statistics in Table 4.1). In terms of lifetime values, the premium with selection increases and slightly flattens out so there is not so much compression at the top of the distribution as there is in the log earnings premium. We saw in subsection 4.3.2 that the public sector workers have greater education than private sector and this is reflected in the the public premia in log earnings and lifetime values when selection into sector is allowed. This confirms the findings of the previous section in which we found a negative public premium when controlling for selection, turning to a small positive premium when selection is allowed, and also echoes the positive selection findings of Dustmann and van Soest (1998).

The Role of Job Mobility

Now we repeat the analysis of the previous section, but this time simulating job as well as earnings mobility, adopting the alternative definition of sector-specific lifetime values as described above. Figure 4.32 shows the public premia based on this new definition of lifetime values, again there is the picture for the "whole sample" and that "with selection". Note that the earnings premium is the same in each case for Figure 4.32 as it is for Figure 4.31 as in each case the comparison is on the basis of earnings flows in the first period.

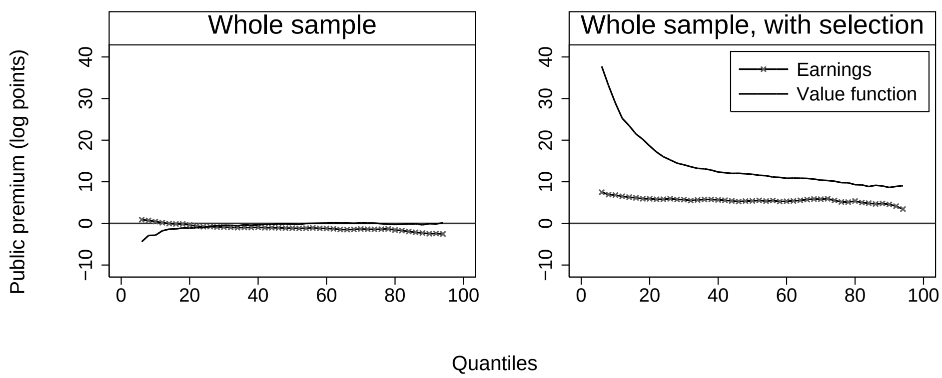


Figure 4.32: **Germany**: The Public Gap

The first thing to notice looking at the left panel of Figure 4.32 is that the public premium in lifetime values which was positive for all points under the 'job for life' assumption averaging around +3 log points, is now negative across the entire distribution, averaging just −0.2 log points in the upper 4 quintiles. It appears that job mobility has a strong effect to equalize lifetime values. The fact that the premium in lifetime values when we use the whole sample of individuals in each sector, is approximately zero for all but the lowest quintile of the distribution suggests that individuals move quickly into their 'natural' sector i.e. the one in which they have highest propensity to be observed in given their unobserved heterogeneity. Even if started in the 'wrong' sector they will quickly move into their 'natural' sector and thus the lifetime values will be calculated over very similar trajectories regardless of the starting sector, and the premium will be close to zero. At the lower end of the distribution individuals experiencing a negative public premium find it harder to get back into the private sector, hence the slightly greater negative premium at the lower end of the distribution. If we look at the $k^m = 1$ individuals – who are predominantly found in the private sector or are unemployed – they spend on average only 2.2 years in the public sector if they start there but for 65.2% of them, the movement is into unemployment when they do move out of the public sector. Clearly this will hit their earnings (we simulate their unemployment income to be 25% of their private sector earnings in Germany) and contribute to the negative premium at the lower part of the distribution. On average it is 7.4 years before they get into the private sector if they have started in the public sector; this compares with just 1.8 years for the $k^m = 2$ or 'private sector attached' individuals. The $k^m = 3$ or 'public attached workers' by constrast remain in the public sector on average for 14.5 years if they start there. It certainly seems that the German labour market is sufficiently mobile to allocate workers to their 'natural' sector quickly even if placed in the 'wrong' sector initially.

The fact that the premium in lifetime values is consistent across the distribution suggests that the greater pay compression in public sector earnings is driven by lower variance of the transitory component of earnings in the public sector. When taking lifetime values these transitory components are averaged, thus cross-sector differences in the variance of this component are mitigated. That the compression in lifetime values is similar in each sector suggests that it is this transitory component – rather than differences in the permanent component of earnings – that is driving the greater public earnings compression. Allowing selection i.e. only including in the private (resp. public) sector simulated lives of individuals who in the real data are observed initially in the private (resp. public) sector, we see again the dramatic selection effect on lifetime values. The negative premium for the lowest part of the distribution is turned into a large positive premium, and even excluding the lowest quintile, the public premium in lifetime values averages +11.5 log points.

4.6.3 Netherlands

The Role of Earnings Mobility

Beginning again with the 'job for life' assumption simulations, Figure 4.33 shows the public premium both in terms of log earnings flows and log lifetime values by percentiles in their respective distributions for the Netherlands. Looking first at the "whole sample" picture, we can see that in terms of earnings flows, there is initially a near zero public premium but that this steadily falls as we move up the percentiles of the distribution such at the top of the distribution the public premium is -7.9 log points. This again is consistent with there being greater pay compression in the public sector, as was illustrated for the Netherlands in the earnings results. Again the picture for lifetime values is similar, the curve shifted up by around 2.5 to 3 log points. The public-premium in lifetime values is initially around $+5$ log points but falling consistently, and becomes negative at exactly the mid-point of the distribution. This suggests that at the lower end of the distribution, the negative public premium in earnings is offset by the higher returns to experience in the public sector such that the public sector enjoys a premium in lifetime values in the first part of the distribution. However, as we move up the percentiles, the public sector earnings dis-premium dominates and the public premium in lifetime values is negative – at the top of the distribution, the public premium in lifetime values is -6.3 log points. The relatively steeper slope of the premium in lifetime values suggests that there is more relative public sector compression in lifetime values than in earnings flows and this shapes the lifetime values premium.

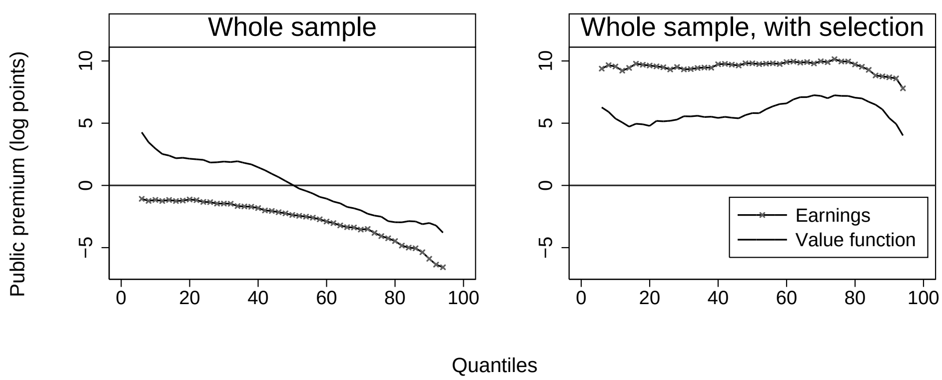


Figure 4.33: **Netherlands**: The Public Gap, 'Jobs for Life'

Turning to the "whole sample, with selection" panel, we see that allowing selection shifts the public premium in earnings up considerably, such that at all points in the distribution there is a positive public premium, averaging at $+9.4$ log points (consistent with figures in the earnings results section and the descriptive analysis). The fact that the premium is largely level across the distribution, only falling in the top quintile suggests that pay compression is only affecting the top of the distribution once selection is accounted for. This implies that there is more positive sorting into the public sector across the middle and upper quintiles compared with the lower quintiles. The changes between the left and right panels of the figure suggests that selection into sector is very important, and this is also shown with respect to lifetime values. The public premium in lifetime values follows a similar transformation, remaining positive across the distribution and largely flat – averaging +5.8 log points despite falling in the top-most quintile. Clearly the selection is positive into the public sector, as the left-panel indicates that without selection there would be a negative premium for the entire distribution of earnings and for the upper half of the distribution of lifetime values. In subsection 4.3.3 we saw that the public sector has a markedly higher educated workforce and this is reflected in the effect of allowing selection on the lifetime value and log earnings public premia.

The Role of Job Mobility

Simulating job as well as earnings mobility, and adopting the alternative definition of sector-specific lifetime values as described in subsection 4.6.1, gives the results shown in Figure 4.34. Again we can note that the earnings premium is the same in each case for Figure 4.34 as it is for Figure 4.33 as in each case the comparison is on the basis of earnings flows in the first period.

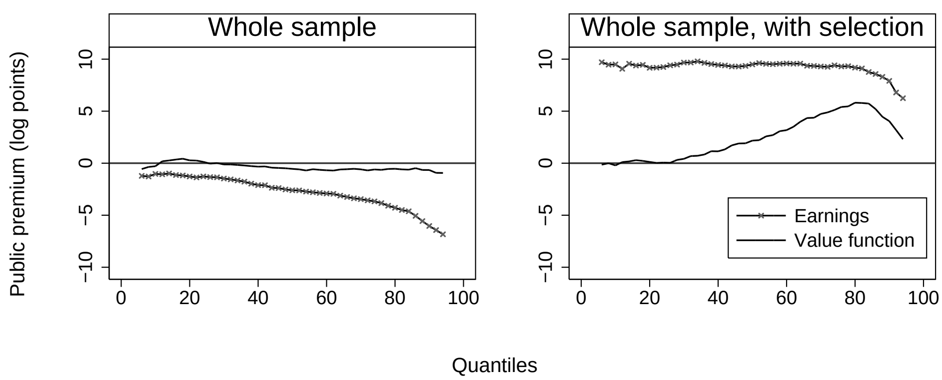


Figure 4.34: **Netherlands**: The Public Gap

Comparing the "whole sample" panel of Figure 4.34 with the corresponding panel of Figure 4.33, we see that the downward sloping premium in lifetime values – starting positive but reversing halfway through the distribution – that characterised the 'job for life' picture, has been replaced with a premium that is almost constant throughout the distribution at just under zero. The suggestion is that job mobility has a strong equalizing effect on the lifetime public premium that averages just -0.4 log points as it varies around zero for the whole distribution. This suggests that in the Netherlands, individuals quickly move into their preferred sector, such that even if they are not initially in that sector, they quickly transit to it, and the lifetime value is therefore calculated over very similar trajectories, irrespective of the starting sector: hence the premium is approximately zero.

If we look specifically at the different mobility types of worker in the data the picture is very clear and as we would expect given the characteristics we explored in section 4.5.3. The $k^m = 1$ mobility types, the 'public sector' type, if initially placed in the private sector spend on average only 1.6 years in that sector before transiting, with 99.4% of the transitions being straight to the public sector. By contrast the 'private sector' types, $k^m = 3$, will remain in the private sector 24.3 years on average if started there, with the majority (70.1% of cases) remaining in the private sector until retirement. The 'lower employment' $k^m = 2$ types who are found in all three states but have more attachment to the private than public sector, will remain in the private sector for 6.8 years if started there, or remain in the public sector for 3.0 years if started there – not surprising to find them in the middle compared to the other types who are so overwhelmingly attached to one or other sector. The corresponding figures for starting in the public sector reflect the same story: 'public sector' types who start there will stay for 16.0 years on average, while 'private sector' types starting in the public sector will remain there for only 1.0 year on average, with the $k^m = 2$ types remaining 3.0 years if started in the public sector. In terms of unemployment replacement value, for the Netherlands we estimate a replacement rate of 50% using the OECD figures. However, as unemployment is rare in the sample and short-lived, differing job loss rates between the sectors do not have a dramatic effect on lifetime value differences. The results from this suggest that the Netherlands labour market is sufficiently efficient that individuals who have a strong attachment to one sector over the other are very quickly allocated into jobs in their preferred sector. Again the fact that we have greater earnings compression but not greater lifetime values compression in the public sector suggests that it is differences in the variance of the transitory component of earnings that influences the observed differences in earnings dispersion between the public and private sectors.

Allowing for selection into sector (right panel), we see that the zero premium in lifetime values remains for the lower part of the distribution, before becoming positive and rising to a peak of +5.8 log points before dropping slightly through the final quintile. This again shows that selection is important, individuals selecting into the sector that maximises their earnings potential.

4.6.4 France

The Role of Earnings Mobility

Starting with the 'job for life' assumption simulations, Figure 4.35 shows for France the public premium both in terms of log earnings flows and log lifetime values by percentiles in their respective distributions. The left panel refers to the "whole sample" picture, i.e. controlling for selection, and we can see that in terms of earnings flows, there is initially a positive public premium of +5.5 log points, but that this declines evenly as we move up the distribution, such that at the very top the premium is just +1.0 log points. Again, this is evidence therefore of there being greater pay compression in the public sector, though it is interesting that in France (in contrast to Germany and the Netherlands) there remains a positive premium in log earnings even at the very top of the distribution, albeit much reduced. The pattern for the log lifetime values premium is very similar, but as with Germany and the Netherlands, the premium is everywhere higher, in this case by approximately 3.4 log points. The fact that there is a positive public premium in earnings across the distribution, and that the returns to experience are generally higher over the lifetime in the public sector contributes to the lifetime values premium at all points of the distribution.

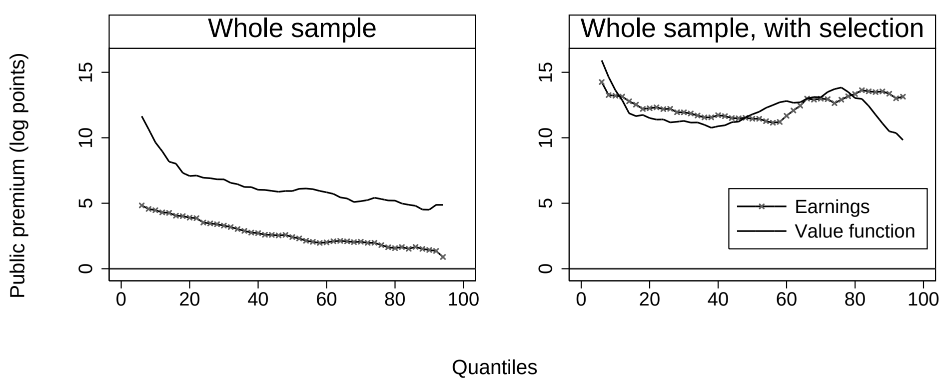


Figure 4.35: **France**: The Public Gap, 'Jobs for Life'

The right panel,"whole sample, with selection", shows that when we allow selection, the public premium in earnings is increased across the entire distribution by an average of 9.8 log points, and becomes somewhat flatter, varying around its average of 12.5 log points (which again concurs with the figures found in the earnings results section and the descriptive analysis of subsection 4.3.4). With respect to the lifetime values public premium, the effect of selection is not as dramatic, the premium increasing by an average of 5.6 log points across the distribution, and also becoming slightly flatter – in fact the log earnings premium and log lifetime values premium almost lie on top of each other when we allow selection. The flattening of the lifetime values premium suggests that in terms of lifetime values there is not as much compression in public sector relative to the private sector as there is for earnings. Recalling from subsection 4.3.4 that the public sector has slightly older and better educated workers, it is to be expected that allowing selection results in the higher premium in earnings and lifetime values and concurs with the findings of the earnings section above.

The Role of Job Mobility

Once again we now turn to the outcomes when we simulate job as well as earnings mobility, adopting the alternative definition of sector-specific lifetime values as described in subsection 4.6.1. Figure 4.36 shows the effect of now simulating job mobility in addition to earnings mobility, on the public premiums in earnings flows and lifetime values.

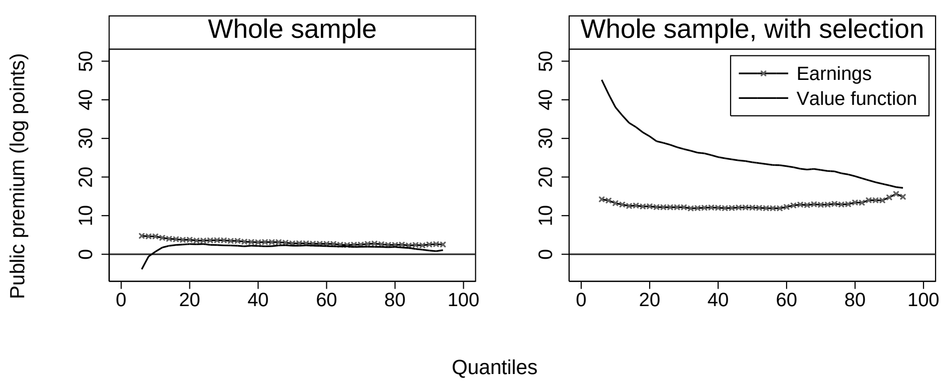


Figure 4.36: **France**: The Public Gap

Looking at the left panel, "whole sample", the first thing to notice from Figure 4.36 is that the public premium in lifetime values which was universally positive and between +14.8 and +4.5 log points, is now initially slightly negative at the lower part of the distribution before turning positive and averaging +1.96 log points for the upper four quintiles. This much smaller positive public premium in lifetime values for the majority of the distribution suggests that in the French labour market it *does* matter which sector is the starting sector, therefore the market is not as efficient in moving people into their 'natural' sector. As was established in section 4.5.4, across the sample there is a positive public premium, and transitions between states are rare – there is not much movement. This means that when individuals are started in the public sector, they will remain there and enjoy that public premium for a considerable length of time before transiting to their 'natural' sector if they are not the 'more public' type ($k^m = 3$). This is backed up by the duration statistics: the $k^m = 1$ ('low employment'), $k^m = 2$ ('more public'), and $k^m = 3$ ('private worker') types spend respectively 20.6, 20.6 and 17.7 years in the public sector if started there. Thus the lifetime value is calculated over quite different trajectories depending on starting sector. This has the effect of preserving the public premium in lifetime values (if individuals quickly transited into their 'natural' sector the premium in lifetime values would be equalized by the job mobility – as it is by-and-large in Germany and the Netherlands). There is also a similar effect at the bottom of the distribution as we found in Germany: that if starting in the public sector some individuals experience a negative public premium in lifetime value. The difference in this case is that it is not that other types quickly move into their 'natural' sector but some take longer – as we have seen, all mobility types remain in the public sector for a relatively long time period if starting there. Rather, the negative premium is because for the 'low employment' and the 'private worker' types, the movement from the public sector when started there is not into the private sector but into unemployment. We calibrate unemployment earnings in France to be 40% of private sector earnings, and as unemployment persistence is high for these types, the effect on lifetime value is high at the bottom of the distribution.

Allowing selection into sector, right-side panel of Figure 4.36, we see two things. Firstly, there is a large premium in lifetime values across all quantiles of the distributions: on average +26.4 log points: selection is clearly very important; the left panel suggests that in general there is a public premium in lifetime values, and this right picture suggests that those who benefit most from the public premium select into that sector. Secondly, the negative premium at the bottom of the distribution is now a large positive premium. This suggests that we observe people in their 'natural' sector in the first period in most cases such that we do not have people spending time in unemployment (and significantly lowering their lifetime value) in between moving into their 'natural' sector.

4.6.5 Italy

The Role of Earnings Mobility

Figure 4.37 shows the public premium both in terms of log earnings flows and log lifetime values by percentiles in their respective distributions, maintaining the 'job for life' assumption. Looking at the "whole sample" picture we can see that in terms of predicted earnings, the public premium is positive, starting at around +7.7 log points, before falling steadily as we move up the percentiles such that at the highest percentile the premium is just +1.3 log points. Again, this pattern of a falling public premium as we move up the distribution is consistent with there being greater pay compression in the public sector. With regard to lifetime values, the picture is somewhat different, with a small negative public premium across the most part of the distribution, turning to a small positive premium only in the highest quintile. Overall the average public premium in lifetime values is −0.5 log points. We know from the earnings results of subsection 4.5.5 that earnings–experience profile is much steeper in the private sector, and this compounds over the lifetime and contributes to the overall negative premium in lifetime values.

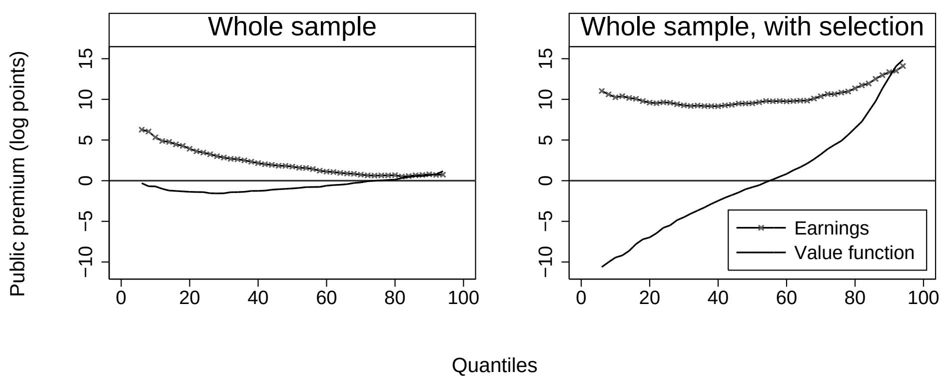


Figure 4.37: **Italy**: The Public Gap, 'Jobs for Life'

Turning to the "whole sample, with selection" panel, we see that when we allow selection, the public premium in earnings is increased across the distribution, the initial downward slope is somewhat flatter, before levelling out across the middle part of the distribution and then rising in the upper quantiles. This suggests that there is a good deal of positive selection into the public sector occurring, with those who have greater earnings potential in the public sector locating in that sector. Moreover we know that the public sector in Italy has more educated workers (14.3% high education, 56.6% medium for the public sector, compared with 6.6% high, 50% medium for the private sector). In terms of lifetime values, the right-panel of Figure 4.37 illustrates that some of the individuals first observed in the public sector, and thus selected into the public sector for this analysis, actually have low lifetime values in the public sector, such that the premium is negative in the lowest half of the distribution. The picture is reversed for the upper half of the distribution: individuals in the top half of the lifetime values distribution in the public sector have higher values than individuals in the top half of the private sector distribution. The slope in the lifetime values premium suggests that lifetime values are more evenly spread amongst those selected into the private sector, with the public sector exhibiting a wider range.

The Role of Job Mobility

Now we repeat the analysis of the previous section, but this time simulating job mobility as well as earnings mobility, adopting the alternative definition of sector-specific lifetime values. Figure 4.38 shows the effect of simulating job mobility in addition to earnings mobility, on the public premiums in earnings flows and lifetime values.

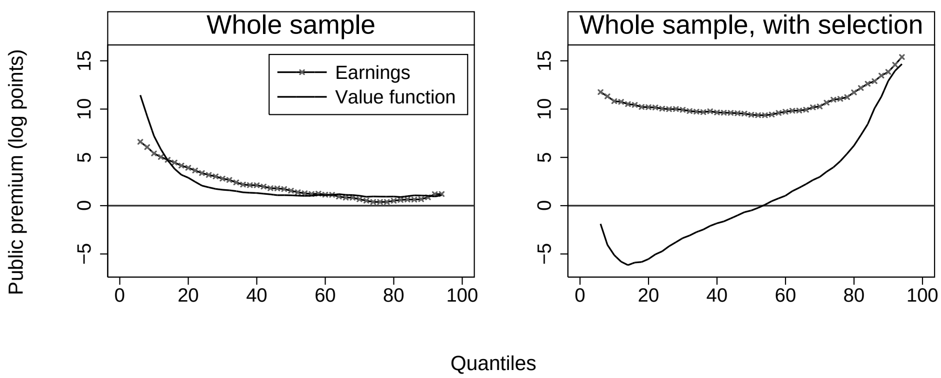


Figure 4.38: **Italy**: The Public Gap

Concentrating first on the “whole sample” panel, the first thing to notice from Figure 4.38 is that the public premium in lifetime values which was small and negative for the most part of the distribution in the ‘job for life’ case, is now positive across the distribution (averaging +2.7 log points). The premium is close to zero for the majority of the distribution, which suggests that the Italian labour market is quite efficient at quickly moving individuals into their ‘natural’ sector such that the trajectories for a ‘lifetime’ in each sector are actually calculated over similar labour market state history, regardless of which sector the individual is initially placed in. Starting in the public sector does seem to have positive benefits for some individuals who have the lowest lifetime values, the greater persistence of earnings in the public sector, coupled with the public premium compounding over a lifetime. For example, the $k^m = 3$ mobility types, who have the lowest employment attachment, if started in the public sector will remain there on average for 8.8 years before transiting, with 32.5% of these transition being to employment in the private sector, only 41.4% being to unemployment. In contrast, when started in the private sector, these mobility types will spend on average only 3.9 years in the private employment before transitting, with 82.5% of those transitions being into unemployment. In unemployment, we simulate individuals to earn only 25% of their private earnings, thus we can see that for these low employment individuals, beginning in the public sector will have an effect on their time spent in unemployment and thus have a significant effect on their lifetime values – this helps to explain why there is such a large public premium in lifetime values at the bottom of the distribution.

When we allow selection into sector, right-side panel of Figure 4.38, we see a very similar picture to the "whole sample, with selection" picture under the 'job for life' assumption – from the twentieth percentile upwards the picture are almost identical. This again suggests that job mobility is sufficiently high in Italy that individuals are quickly sorted into their 'natural' sector and that there is not a great deal of subsequent movement, such that the 'job for life' and the unmobility constrained pictures are not very different. We know that the job loss rates from each sector in Italy are relatively low (2.7% private sector, 1.0% public), and the rate of movement between the sectors is also relatively low (4.8% of workers moving from public to private sector from $t-1$ to t, 1.9% moving the other way), which confirms the picture of employment stability. Job mobility does seem to mitigate the negative public premium in lifetime values at the lowest part of the distribution, reducing it for all in the lowest quintile.

4.6.6 Spain

The Role of Earnings Mobility

Again we begin the analysis by looking at the simulations run under the 'job for life' assumption. Figure 4.39 shows for Spain the public premium both in terms of log earnings flows and log lifetime values by the quantiles of in their respective distributions. The public premium in earnings is positive throughout the distribution – averaging +11.9 log points – initially as much as +19.1 log points, falling to +4.0 log points at the top of the distribution. This downward pattern consistent with the phenomena of greater pay compression in the public sector. We have seen in the earnings results section that in Spain the greater pay compression in the public sector is very pronounced, the standard deviation of log earnings lower by 3.4 log points in the public compared with the private sector. With respect to lifetime values, the public premium is positive across the distribution – averaging +7.8 log points – and is almost flat, with a little variation, up as high as +10.7 log points at the bottom of the distribution, though it is never below +7.1 log points at any point. The flatter curve suggests much lower public sector compression in terms of lifetime values than is exhibited in earnings. This may be explained by the higher returns to experience in the private sector, at least for the first half of the working lifetime, reducing the public earnings premium.

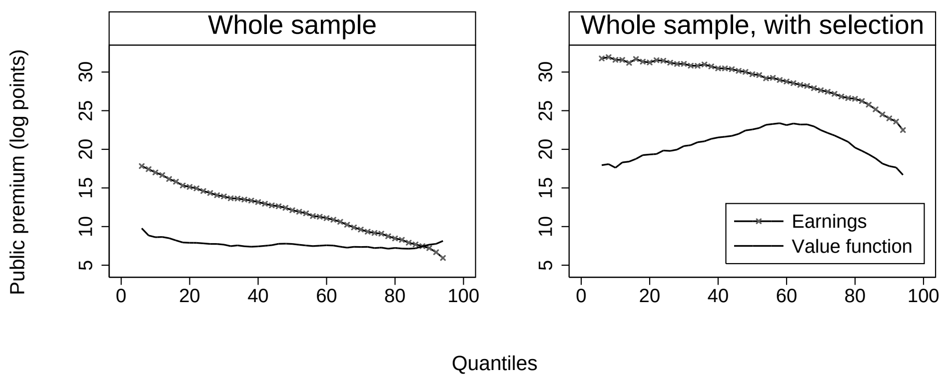


Figure 4.39: **Spain**: The Public Gap, 'Jobs for Life'

The "whole sample, with selection" (right) panel, shows that when we allow selection, the public premium in earnings is increased across the entire distribution by more than +10 log points, now averaging across the distribution at +28.8 log points (comparable with the findings in the results and descriptive analysis sections). The downward slope in the earnings premium is not quite so high when allowing for selection, the premium falling from +31.6 log points to +19.3 log points as we move from the lowest quantile to the highest. In terms of lifetime values, again allowing selection results in a higher public premium than in the case where we control for the selection, the premium is higher than it was at the lower end of the distribution (+14.4

compared with +10.7), and then rises up to +23.4 log points in the middle of the distribution before falling again down to around +15.7 log points at the top. Thus both in terms of earnings and lifetime values, selection effects are important: this is not surprising given the descriptive analysis of subsection 4.3.6 which showed that the public sector has a much greater proportion of high education workers (51.0% public, compared to 28.7%, private), and that the public sector also has more experienced workers. However, the "whole sample" picture does suggest that even controlling for selection, there is across the distribution, a large public premium in earnings and lifetime values – again echoing Lassibille (1998).

The Role of Job Mobility

We now look at the simulations when job mobility is simulated in addition to earnings mobility, adopting the alternative definition of sector-specific lifetime values; Figure 4.40 shows the public premiums in earnings flows and lifetime values.

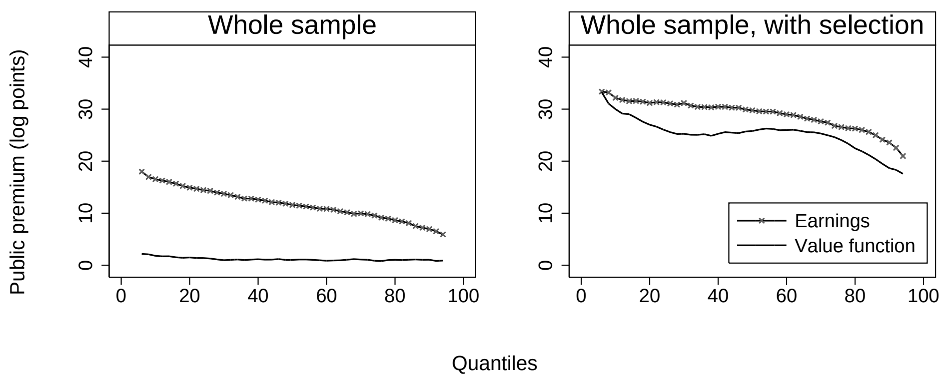


Figure 4.40: **Spain**: The Public Gap

The "whole sample" (left) panel of Figure 4.40 shows that the relatively large public premium in lifetime values under the 'job for life' assumption is almost totally eroded away when we allow job mobility, averaging just +1.1 log points over the distribution. This suggests that the Spanish labour market is mobile, such that whichever sector workers are initially placed in, they soon move to their 'natural' sector and therefore the trajectories over which their lifetime values are calculated in each (starting) sector will be very similar. This is illustrated by the time each type of worker spends in a sector when simulated to begin there: the 'public worker' type $k^m = 1$ will remain in the pubic sector for 13.2 years on average if started there, compared to 2.3 years for the $k^m = 2$ 'low employment' types, and only 1.5 years for the 'private worker' types, $k^m = 3$.

Conversely, if started in the private sector, the 'public worker' types will remain there only 4.0 years on average, 'low employment' 3.8 years, whereas the 'private worker' types will remain for 17.2 years on average. Thus workers either remain a long time in their initial sector if in their 'natural' sector to begin with, or move quite quickly out if they are in the 'wrong' sector. As with Germany and the Netherlands, for Spain the greater earnings compression witnessed in the public sector, is not evident in the lifetime values. This suggests that the relative compression in the public sector earnings is due to a lower variance of the transitory component of earnings for the public sector, rather than lower permanent inequality in the public sector. Allowing selection into sector, the right-side panel of Figure 4.40, the public premium in lifetime values is greatly increased, averaging +25.4 log points across the distribution. As with earnings, there is evidence of greater compression in the public sector, the lifetime value premium starting above its average before stabilizing at the average over the middle quintiles of the distribution, and then falling in the upper quantiles, ending at +16.7 log points in the very highest quantile. The evidence from this right panel suggests that Spain's labour market sorts those with higher public sector earnings potential into this sector, such that the public premium allowing selection is very high.

4.6.7 Portugal

The Role of Earnings Mobility

As ever, we start by looking at the simulations run under the 'job for life' assumption, with Figure 4.41 illustrating for Portugal the public premium both in terms of log earnings flows and log lifetime values by the quantiles of in their respective distributions. Unlike the other countries under consideration, Portugal exhibits a *lower* degree of pay compression in the public sector, and as such we might expect any public premium in earnings to be smaller at the lower end of the distribution but rising. This is exactly what we see in the "whole sample" panel of Figure 4.41. Initially the public premium is −11.9 log points, but this rises as we move up the distribution, becoming positive in the second quintile, and continues to rise steadily, such that it is +16.0 log points at the top of the distribution. With regard to lifetime values, we see the same compression pattern: the public premium in lifetime values rises steadily as we move up the distribution, but unlike earnings, the lifetime value premium is always positive, starting at

+5.3 log points and rising as high as +22.6 before dropping slightly at the very highest decile. The premium in earnings in the public sector is compounded by the greater returns to experience in the public sector such that the premium in lifetime values is even greater than in earnings under the 'job for life' assumption.

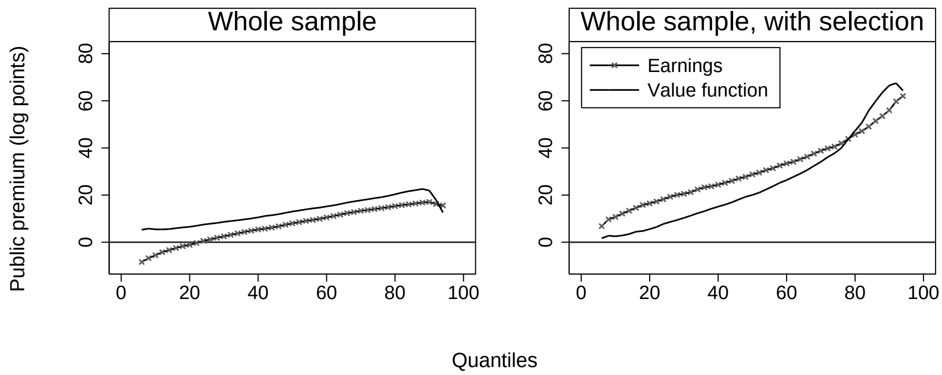


Figure 4.41: **Portugal**: The Public Gap, 'Jobs for Life'

Looking now at the "whole sample, with selection" panel, we see that allowing selection, the public premium in earnings is increased across the entire distribution, the average across the distribution increasing from +6.8 log points, to +30.7 log points. This average is pulled up by a larger increase at the top of the distribution than the more modest +14.3 log point increase at the very bottom of the distribution. As the public sector contains markedly older and more experienced workers, and with more high and medium educated workers, it is not surprising that even simulating 'jobs for life' the picture not controlling selection is so positive in favour of the public sector.

The Role of Job Mobility

Again we next turn to the simulation results when job mobility is simulated in addition to earnings mobility, adopting the alternative definition of sector-specific lifetime values as described in section 4.6.1. The public premium in earnings flows and lifetime values is shown in Figure 4.42.

The left-side panel,"whole sample", of Figure 4.42 shows that for all but the very lowest quantiles, the public premium in lifetime values is drastically reduced when we simulate movement between sectors. Beginning at around the same level as when we do not simulate job mobility (+5.4 log points) the premium then falls, averaging just +1.7 log points over the entire distribution. As with many of the countries, it appears that the labour market is sufficiently

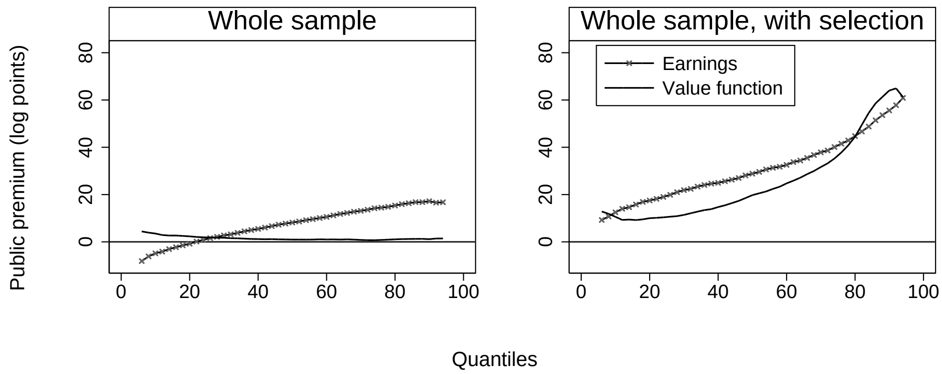


Figure 4.42: **Portugal**: The Public Gap

efficient to sort workers into the sector where they have the highest employability and earnings potential, quickly enough that regardless of the starting sector, individuals have a similar labour history trajectory, hence there is not much of a premium for the public sector. As we saw in subsection 4.5.7, the $k^m = 1$ mobility types are very much 'public workers' whereas the $k^m = 2$ mobility types are strongly attached to the private sector. The efficiency of the labour market sorting can be seen in the duration each of these types spends in the public (resp. private) sector when simulated to start there: the 'public workers' remain in the public sector for an average of 10.3 years when started there, compared with just 1.3 years for the 'private workers'. In contrast, when starting in the private sector the 'public' types will remain on average 4.4 years, while the 'private' type will remain 22.3 years on average. Clearly the relative speed with which workers become reallocated contributes substantially to the erosion of the high public premium in lifetime values. As with Germany, the Netherlands and Spain, for Portugal the fact that the greater earnings compression in *private* sector earnings is not witnessed in lifetime values would tend to suggest that the difference in earnings inequality between the sectors owes to a greater variance of the transitory component of earnings in the public sector. Over the lifetime these transitory components are averaged out and thus the difference in compression is not apparent in lifetime values.

When we allow selection into sector, the right panel of Figure 4.42 illustrates that those with the higher public premiums select into this sector, shifting the lifetime value premium upwards, and mirroring very closely the 'job for life' selection panel. This indicates that once in their 'natural' sector individuals remain in employment there, such that the simulation of mobility does not much alter the picture – the job loss rates are low for each sector in Portugal, and it appears that as a result the 'job for life' assumption is not far from the reality when we do not control for selection.

4.6.8 ***Summary of The Public Pay Gap in Lifetime Values Results***

The conclusions of this analysis of public-private pay gaps in lifetime values can be summarised as follows:

- **Germany**: Under the 'job for life' assumption there is on average a small positive premium in lifetime values averaging out to be +2.5 log points across the distribution. Simulating movement of individuals between job states and into/out of unemployment, the premium is approximately zero.

- **The Netherlands**: Under the 'job for life' assumption there is a small positive public premium, averaging +2.0 log points in the first half of the distribution, but a small negative premium, averaging −2.1 log points in the top of half of the distribution. Simulating movement between labour market states, the premium is approximately zero across the distribution.

- **France**: Assuming jobs for life in either sector, the public premium in lifetime values is high and positive across the distribution, averaging +6.3 log points. Simulating movement into/out of each sector, the premium is small and positive across the majority of the distribution, averaging +1.8 log points.

- **Italy**: In the 'job for life' scenario, there is a small, negative public premium in lifetime values across the most part of the distribution, averaging at −0.6 log points. Allowing for movement between the labour market sectors, this premium becomes positive or zero, averaging at +2.7 log points.

- **Spain**: Under the 'job for life' assumption there is a considerable public premium in lifetime values, stable across the distribution and averaging +7.7 log points. Simulating movement between the employment sectors and into/out of unemployment this shifts down to a stable value around +1.1 log points.

- **Portugal**: Assuming jobs for life in either sector, the public premium in lifetime values is always positive and rising as we move up the distribution, averaging to be +13.0 log points. Simulating movement between labour market states reduces this premium considerably and is close to zero for the most part of the distribution, averaging +1.7 log points.

The over-arching conclusion from this is that movement between labour market states has a substantial effect on the public premium in lifetime values. Allowing movement between the sectors, we conclude that for France and Italy there is a positive public premium in potential lifetime values across almost all of the distribution. For Germany, the Netherlands, Spain and Portugal however we conclude that the labour market is sufficiently mobile within each country that even if started in the 'wrong' sector, a worker will transit to their 'natural' sector in short enough time that their lifetime value whichever sector they start in is approximately the same. This results in an approximately zero public premium in lifetime values for these countries. We can also see that for countries in which there is greater earnings compression in the public sector – which is all of them to a greater or lesser extent, bar Portugal – this greater compression is not evidenced in the lifetime values premium which is generally uniform as we move up the distribution. This suggests that the greater earnings compression in the public sector may be due to a greater variance in the transitory component of earnings in the private sector – increasing the cross-sectional pay dispersion in this sector. Over a lifetime the transitory components are averaged out, thus the dispersion is reduced. If the differences in earnings compression were being driven by differences in the variance of the permanent component of earnings, we would expect that to remain when we take lifetime values. That the compression is not witnessed in the lifetime values premium suggests that it is the difference in transitory earnings between the sectors that drives the compression in the public sector.

4.7 Conclusions

Regardless of country, the literature on public-private pay differences tends to focus on cross-sectional differences in earnings, and the extent to which they can be 'explained away' by selection. However, as the sectors also differ in terms of earnings mobility and job mobility, these factors also need to be taken into account in any assessment of the public-pay gap. In a dynamic environment forward looking agents care about their job security and earnings dynamics and anticipate that these differ between the sectors and this will affect their assessment of the lifetime value of potential employment in either sector. To derive a more informative comparison of pay in the public and private sectors, we apply a flexible model of earnings and employment dynamics, where the individual earnings and employment trajectories are conditioned by unobserved as well as observed individual heterogeneity.

We estimate the model on ECHP data for Germany, the Netherlands, France, Italy, Spain and Portugal. In each of the countries we are able to fit well the observed cross-sectional distribution of workers into sectors and the cross sectional earnings distributions, and importantly also the patterns of labour market mobility and earnings mobility. Then within each country, using the estimated coefficients from the model for that country, we simulate individual working life histories and earnings for all of the individuals in the country's dataset and compute the present discounted values of their lifetime earnings flows and compare these across sectors.

The main findings are as follows. Firstly, that if individuals were to remain in employment for all of their working life in a sector then the average public premium in lifetime values would be positive in Germany, France, Spain and Portugal (and large in the latter two cases), whereas in Italy and the Netherlands the average public premium would be slightly below zero. If however, individuals are allowed to move between the sectors in the simulations, then the average public premium becomes approximately zero for Germany, the Netherlands, Spain and Portugal. In Italy and France there is on average a small positive public premium in lifetime values when job mobility is simulated. The conclusion we draw from this is that in each country there is sufficient job mobility, that individuals are quickly sorted into their 'natural' sector. The finding that the public premium in lifetime values, in most of the countries, remains uniform and close to zero as we move up the percentiles of the distribution suggests that where we do observe somewhat greater earnings compression in the public sector, it is due to a lower variance in the transitory component of earnings in the public sector.

4.8 Descriptive Tables

Dependent variable: log monthly earnings	Specification number ...				
	1	2	3	4	5
Constant	8.452 (0.007)	8.570 (0.020)	8.614 (0.022)	8.205 (0.021)	8.229 (0.022)
Public	0.052 (0.014)	0.014 (0.012)	−0.188 (0.037)	−0.012 (0.011)	−0.132 (0.036)
Experience (years/10)	—	0.114 (0.019)	0.085 (0.021)	0.272 (0.018)	0.245 (0.019)
Experience^2 (years^2/100)	—	−0.021 (0.004)	−0.017 (0.005)	−0.056 (0.004)	−0.050 (0.004)
Medium ed.	—	−0.320 (0.016)	−0.332 (0.019)	—	—
Low ed.	—	−0.347 (0.018)	−0.348 (0.021)	—	—
Public x Medium ed.	—	—	0.052 (0.029)	—	0.003 (0.023)
Public x Low ed.	—	—	−0.025 (0.053)	—	0.064 (0.033)
Public x Experience	—	—	0.129 (0.038)	—	0.121 (0.030)
Public x Experience^2	—	—	−0.018 (0.009)	—	−0.024 (0.006)
Fixed Effects	No	No	No	Yes	Yes
R^2	0.004	0.173	0.178	0.042	0.046
Observations	18183	18183	18183	18183	18183
Individuals	2963	2963	2963	2963	2963

Notes: All years pooled.
Specifications 1, 2, and 3: OLS. Specifications 4 and 5: within estimator.
Reference categories are "Private sector job" and "High education".
Standard Errors in parentheses.

Table 4.1: **Germany** Public-Private differences: mean earnings

	Private sector	Public sector
	earnings quintile at t	earnings quintile at t
earnings quintile at $t-1$	$\begin{pmatrix} 76.1 & 18.7 & 3.3 & 1.8 & 0.2 \\ 17.7 & 56.8 & 19.8 & 5.1 & 0.6 \\ 3.5 & 19.9 & 53.8 & 20.2 & 2.7 \\ 1.0 & 4.1 & 20.1 & 60.1 & 14.7 \\ 0.1 & 0.8 & 2.0 & 12.8 & 84.3 \end{pmatrix}$	$\begin{pmatrix} 79.8 & 17.0 & 2.1 & 0.7 & 0.4 \\ 9.8 & 64.3 & 22.7 & 2.5 & 0.7 \\ 2.0 & 17.8 & 60.1 & 19.1 & 1.1 \\ 0.5 & 1.0 & 15.0 & 70.7 & 12.9 \\ 0.2 & 0.3 & 1.5 & 10.6 & 87.6 \end{pmatrix}$
	residual quintile at t	residual quintile at t
residual quintile at $t-1$	$\begin{pmatrix} 78.1 & 16.4 & 3.7 & 1.3 & 0.6 \\ 16.5 & 57.5 & 18.7 & 6.0 & 1.3 \\ 3.2 & 19.6 & 52.8 & 20.5 & 3.9 \\ 1.2 & 4.6 & 21.7 & 56.0 & 16.6 \\ 0.3 & 1.1 & 3.9 & 15.6 & 79.1 \end{pmatrix}$	$\begin{pmatrix} 79.8 & 17.2 & 1.8 & 0.9 & 0.4 \\ 11.9 & 65.3 & 19.3 & 3.0 & 0.5 \\ 0.8 & 14.8 & 63.7 & 18.8 & 1.9 \\ 0.8 & 2.2 & 18.7 & 63.7 & 14.6 \\ 0.2 & 0.7 & 2.0 & 12.6 & 84.6 \end{pmatrix}$

Notes: Sector-specific earnings quintiles.
Top panel: unconditional raw earnings quintiles;
Lower panel: quintiles of residual from regression of log earnings on experience, experience^2 and education.

Table 4.2: **Germany** Public-Private differences: mobility of earnings ranks

Dependent variable: log monthly earnings					
	Specification number ...				
	1	2	3	4	5
Constant	8.692	8.621	8.638	8.262	8.252
	(0.007)	(0.020)	(0.023)	(0.022)	(0.023)
Public	0.094	−0.033	−0.097	−0.002	0.045
	(0.014)	(0.012)	(0.048)	(0.007)	(0.031)
Experience (years/10)	—	0.311	0.321	0.382	0.390
		(0.019)	(0.021)	(0.019)	(0.020)
Experience^2 (years^2/100)	—	−0.052	−0.055	−0.065	−0.067
		(0.004)	(0.005)	(0.004)	(0.004)
Medium ed.	—	−0.344	−0.374	—	—
		(0.013)	(0.017)		
Low ed.	—	−0.448	−0.466	—	—
		(0.017)	(0.019)		
Public x Medium ed.	—	—	0.095	—	0.020
			(0.025)		(0.016)
Public x Low ed.	—	—	0.033	—	0.009
			(0.041)		(0.020)
Public x Experience	—	—	−0.017	—	−0.042
			(0.043)		(0.025)
Public x Experience^2	—	—	0.008	—	0.006
			(0.009)		(0.005)
Fixed Effects	No	No	No	Yes	Yes
R^2	0.015	0.356	0.360	0.118	0.120
Observations	13614	13614	13614	13614	13614
Individuals	2175	2175	2175	2175	2175

Notes: All years pooled.
Specifications 1, 2, and 3: OLS. Specifications 4 and 5: within estimator.
Reference categories are "Private sector job" and "High education".
Standard Errors in parentheses.

Table 4.3: **Netherlands** Public-Private differences: mean earnings

	Private sector					Public sector				
	earnings quintile at t					earnings quintile at t				
earnings quintile at $t-1$	77.9	18.3	3.2	0.5	0.1	83.8	15.1	1.1	0.0	0.0
	13.9	64.3	18.1	3.3	0.5	13.3	67.2	17.1	2.0	0.4
	1.8	15.1	64.8	16.6	1.9	0.8	13.5	67.7	16.2	1.8
	0.5	1.6	13.2	71.2	13.5	0.2	2.0	14.3	72.3	11.3
	0.0	0.4	0.8	9.5	89.4	0.0	0.0	0.9	9.8	89.3
	residual quintile at t					residual quintile at t				
residual quintile at $t-1$	78.0	16.3	3.8	1.3	0.6	86.4	11.7	1.5	0.4	0.0
	16.7	60.4	18.3	3.9	0.7	14.8	65.9	16.8	2.1	0.4
	2.9	18.8	57.1	17.6	3.6	1.6	17.0	62.1	17.0	2.2
	0.8	3.1	18.2	61.5	16.4	0.6	2.2	19.0	64.7	13.5
	0.3	0.5	2.1	15.9	81.2	0.2	0.0	2.1	14.4	83.3

Notes: Sector-specific earnings quintiles.
Top panel: unconditional raw earnings quintiles;
Lower panel: quintiles of residual from regression of log earnings on experience, experience^2 and education.

Table 4.4: **Netherlands** Public-Private differences: mobility of earnings ranks

Dependent variable: log monthly earnings					
	Specification number ...				
	1	2	3	4	5
Constant	9.409 (0.011)	9.364 (0.024)	9.378 (0.028)	8.882 (0.029)	8.881 (0.031)
Public	0.125 (0.020)	0.041 (0.015)	−0.014 (0.056)	−0.014 (0.026)	0.038 (0.069)
Experience (years/10)	—	0.396 (0.024)	0.400 (0.027)	0.402 (0.025)	0.416 (0.027)
Experience^2 (years^2/100)	—	−0.062 (0.006)	−0.064 (0.006)	−0.053 (0.005)	−0.059 (0.005)
Medium ed.	—	−0.521 (0.020)	−0.526 (0.024)	—	—
Low ed.	—	−0.696 (0.021)	−0.729 (0.026)	—	—
Public x Medium ed.	—	—	0.011 (0.039)	—	−0.101 (0.064)
Public x Low ed.	—	—	0.131 (0.044)	—	−0.006 (0.071)
Public x Experience	—	—	−0.014 (0.056)	—	−0.054 (0.046)
Public x Experience^2	—	—	0.008 (0.013)	—	0.021 (0.009)
Fixed Effects	No	No	No	Yes	Yes
R^2	0.017	0.419	0.423	0.098	0.102
Observations	12773	12773	12773	12773	12773
Individuals	2217	2217	2217	2217	2217

Notes: All years pooled.
Specifications 1, 2, and 3: OLS. Specifications 4 and 5: within estimator.
Reference categories are "Private sector job" and "High education".
Standard Errors in parentheses.

Table 4.5: **France** Public-Private differences: mean earnings

	Private sector					Public sector				
	earnings quintile at t					earnings quintile at t				
earnings quintile at $t-1$	71.4	22.9	4.7	0.7	0.2	74.6	21.4	3.2	0.7	0.0
	17.3	58.3	21.5	2.9	0.1	13.0	62.3	21.7	2.1	0.9
	2.8	16.5	58.7	20.4	1.7	1.6	12.9	62.4	20.0	3.1
	0.9	1.9	14.2	69.2	13.9	0.9	2.1	10.3	71.7	15.0
	0.2	0.5	1.6	10.1	87.7	0.0	0.6	2.6	9.6	87.2
	residual quintile at t					residual quintile at t				
residual quintile at $t-1$	75.9	18.9	3.4	1.4	0.5	75.2	20.4	3.3	0.9	0.2
	18.2	54.4	22.1	4.3	1.1	14.3	62.6	19.0	4.0	0.4
	3.0	21.1	53.5	21.0	1.4	3.2	15.8	55.0	21.9	4.1
	1.3	4.6	18.6	59.9	15.5	1.3	3.0	17.7	60.3	17.8
	0.6	1.7	2.4	13.9	81.4	0.2	1.1	3.2	14.1	81.5

Notes: Sector-specific earnings quintiles.
Top panel: unconditional raw earnings quintiles;
Lower panel: quintiles of residual from regression of log earnings on experience, experience^2 and education.

Table 4.6: **France** Public-Private differences: mobility of earnings ranks

Dependent variable: log monthly earnings	Specification number ...				
	1	2	3	4	5
Constant	7.933	8.070	8.011	7.582	7.566
	(0.006)	(0.025)	(0.030)	(0.024)	(0.026)
Public	0.086	−0.012	0.210	0.009	0.054
	(0.011)	(0.009)	(0.052)	(0.009)	(0.047)
Experience (years/10)	—	0.263	0.310	0.282	0.296
		(0.015)	(0.017)	(0.020)	(0.022)
Experience^2 (years^2/100)	—	−0.042	−0.051	−0.041	−0.043
		(0.003)	(0.004)	(0.004)	(0.005)
Medium ed.	—	−0.409	−0.389	—	—
		(0.026)	(0.031)		
Low ed.	—	−0.571	−0.572	—	—
		(0.025)	(0.031)		
Public x Medium ed.	—	—	−0.039	—	0.038
			(0.047)		(0.039)
Public x Low ed.	—	—	0.034	—	0.028
			(0.050)		(0.040)
Public x Experience	—	—	−0.174	—	−0.057
			(0.035)		(0.030)
Public x Experience^2	—	—	0.030	—	0.008
			(0.007)		(0.006)
Fixed Effects	No	No	No	Yes	Yes
R^2	0.018	0.314	0.321	0.043	0.044
Observations	15361	15361	15361	15361	15361
Individuals	2645	2645	2645	2645	2645

Notes: All years pooled.
Specifications 1, 2, and 3: OLS. Specifications 4 and 5: within estimator.
Reference categories are “Private sector job” and “High education”.
Standard Errors in parentheses.

Table 4.7: **Italy** Public-Private differences: mean earnings

	Private sector					Public sector				
	earnings quintile at t					earnings quintile at t				
earnings quintile at $t-1$	62.3	23.9	8.8	4.5	0.5	66.8	20.9	7.3	4.3	0.7
	20.1	45.4	23.9	8.7	1.9	19.4	48.4	22.0	8.4	1.9
	6.8	20.1	44.6	22.6	6.0	5.6	21.8	47.6	21.8	3.1
	1.9	6.7	20.4	52.2	18.9	2.5	6.9	21.8	52.3	16.5
	0.6	0.6	4.3	17.3	77.2	0.5	1.5	3.2	15.2	79.7
	residual quintile at t					residual quintile at t				
residual quintile at $t-1$	62.0	21.1	9.9	4.9	2.1	71.6	16.6	7.4	3.2	1.3
	21.0	44.6	22.1	9.4	2.9	20.0	50.4	19.2	8.0	2.5
	8.3	23.3	38.9	20.7	8.9	4.6	22.3	47.3	20.2	5.6
	3.5	9.0	23.9	46.6	17.1	1.9	8.7	23.2	47.6	18.6
	1.6	2.7	5.7	18.9	71.2	1.0	1.5	4.2	21.2	72.0

Notes: Sector-specific earnings quintiles.
Top panel: unconditional raw earnings quintiles;
Lower panel: quintiles of residual from regression of log earnings on experience, experience^2 and education.

Table 4.8: **Italy** Public-Private differences: mobility of earnings ranks

Dependent variable: log monthly earnings					
	Specification number ...				
	1	2	3	4	5
Constant	12.243 (0.009)	12.064 (0.023)	12.033 (0.025)	11.731 (0.033)	11.699 (0.035)
Public	0.267 (0.018)	0.097 (0.016)	0.270 (0.058)	−0.002 (0.014)	0.162 (0.051)
Experience (years/10)	—	0.371 (0.022)	0.393 (0.024)	0.371 (0.027)	0.395 (0.030)
Experience^2 (years^2/100)	—	−0.053 (0.005)	−0.057 (0.005)	−0.040 (0.005)	−0.044 (0.006)
Medium ed.	—	−0.289 (0.020)	−0.273 (0.024)	—	—
Low ed.	—	−0.503 (0.018)	−0.496 (0.021)	—	—
Public x Medium ed.	—	—	−0.059 (0.038)	—	−0.050 (0.038)
Public x Low ed.	—	—	−0.022 (0.037)	—	−0.053 (0.031)
Public x Experience	—	—	−0.130 (0.052)	—	−0.098 (0.041)
Public x Experience^2	—	—	0.023 (0.011)	—	0.013 (0.008)
Fixed Effects	No	No	No	Yes	Yes
R^2	0.067	0.352	0.353	0.066	0.069
Observations	13854	13854	13854	13854	13854
Individuals	2538	2538	2538	2538	2538

Notes: All years pooled.
Specifications 1, 2, and 3: OLS. Specifications 4 and 5: within estimator.
Reference categories are "Private sector job" and "High education".
Standard Errors in parentheses.

Table 4.9: **Spain** Public-Private differences: mean earnings

	Private sector	Public sector
	earnings quintile at t	earnings quintile at t
earnings quintile at $t-1$	$\begin{pmatrix} 53.3 & 30.2 & 13.0 & 3.2 & 0.3 \\ 21.4 & 40.5 & 29.4 & 8.0 & 0.7 \\ 7.3 & 23.3 & 41.5 & 25.0 & 3.0 \\ 1.7 & 5.6 & 17.6 & 54.0 & 21.1 \\ 0.3 & 0.4 & 1.8 & 15.6 & 81.9 \end{pmatrix}$	$\begin{pmatrix} 68.5 & 26.8 & 4.2 & 0.5 & 0.0 \\ 21.1 & 53.2 & 22.6 & 2.9 & 0.2 \\ 3.3 & 18.6 & 52.7 & 23.6 & 1.9 \\ 0.2 & 2.2 & 20.8 & 55.5 & 21.4 \\ 0.2 & 0.6 & 2.2 & 18.1 & 78.9 \end{pmatrix}$
	residual quintile at t	residual quintile at t
residual quintile at $t-1$	$\begin{pmatrix} 63.7 & 23.7 & 8.4 & 3.6 & 0.6 \\ 20.8 & 41.6 & 25.6 & 9.1 & 2.5 \\ 5.1 & 25.5 & 37.6 & 25.9 & 5.9 \\ 2.7 & 6.9 & 23.5 & 44.8 & 22.1 \\ 0.5 & 2.2 & 4.7 & 18.9 & 73.7 \end{pmatrix}$	$\begin{pmatrix} 72.5 & 21.6 & 5.1 & 0.4 & 0.4 \\ 22.2 & 46.9 & 25.1 & 5.1 & 0.8 \\ 5.6 & 23.0 & 46.5 & 20.8 & 4.2 \\ 0.6 & 8.1 & 21.6 & 50.9 & 19.0 \\ 0.6 & 0.4 & 2.7 & 21.3 & 75.0 \end{pmatrix}$

Notes: Sector-specific earnings quintiles.
Top panel: unconditional raw earnings quintiles;
Lower panel: quintiles of residual from regression of log earnings on experience, experience^2 and education.

Table 4.10: **Spain** Public-Private differences: mobility of earnings ranks

Dependent variable: log monthly earnings	Specification number ...				
	1	2	3	4	5
Constant	11.727 (0.009)	12.246 (0.037)	12.325 (0.049)	11.277 (0.030)	11.281 (0.031)
Public	0.295 (0.024)	0.109 (0.017)	−0.172 (0.073)	−0.011 (0.012)	−0.010 (0.065)
Experience (years/10)	—	0.361 (0.020)	0.339 (0.022)	0.393 (0.025)	0.392 (0.026)
Experience^2 (years^2/100)	—	−0.063 (0.004)	−0.061 (0.005)	−0.054 (0.005)	−0.054 (0.005)
Medium ed.	—	−0.653 (0.040)	−0.715 (0.051)	—	—
Low ed.	—	−0.995 (0.035)	−1.047 (0.047)	—	—
Public x Medium ed.	—	—	0.137 (0.078)	—	−0.030 (0.053)
Public x Low ed.	—	—	0.086 (0.068)	—	−0.030 (0.047)
Public x Experience	—	—	0.136 (0.052)	—	0.004 (0.034)
Public x Experience^2	—	—	−0.019 (0.011)	—	0.003 (0.007)
Fixed Effects	No	No	No	Yes	Yes
R^2	0.077	0.457	0.461	0.080	0.081
Observations	13320	13320	13320	13320	13320
Individuals	2193	2193	2193	2193	2193

Notes: All years pooled.
Specifications 1, 2, and 3: OLS. Specifications 4 and 5: within estimator.
Reference categories are "Private sector job" and "High education".
Standard Errors in parentheses.

Table 4.11: **Portugal** Public-Private differences: mean earnings

	Private sector					Public sector				
	earnings quintile at t					earnings quintile at t				
earnings quintile at $t-1$	69.3	20.3	7.0	2.7	0.7	82.6	15.0	1.7	0.2	0.5
	14.4	58.7	18.5	6.5	1.9	11.5	70.3	15.4	2.3	0.5
	3.7	17.6	59.1	16.8	2.8	1.9	13.6	70.0	13.8	0.7
	1.4	3.6	14.3	68.5	12.2	0.0	1.1	8.4	78.5	12.0
	0.6	0.7	2.3	11.2	85.2	0.0	0.0	0.2	7.6	92.2
	residual quintile at t					residual quintile at t				
residual quintile at $t-1$	75.1	17.0	4.5	2.4	1.0	78.3	17.1	2.8	1.4	0.5
	17.4	54.8	18.8	6.8	2.4	14.3	63.8	18.4	2.6	0.9
	3.4	20.7	54.9	16.3	4.8	2.1	14.5	67.0	13.4	3.0
	1.6	4.3	18.5	60.5	15.4	1.3	2.0	11.6	70.8	14.3
	0.8	1.3	3.7	15.4	78.9	0.0	0.5	0.9	10.8	87.8

Notes: Sector-specific earnings quintiles.
Top panel: unconditional raw earnings quintiles;
Lower panel: quintiles of residual from regression of log earnings on experience, experience^2 and education.

Table 4.12: **Portugal** Public-Private differences: mobility of earnings ranks

4.9 Results Tables

	Observed			Predicted			
state at $t-1$	state at t			state at t			
↓	private	public	unemp.	private	public	unemp.	
Private	94.1	1.2	4.5	95.2	1.1	3.6	Max distance: 5.3
Public	6.6	91.1	2.1	6.1	91.9	1.9	Max distance, 2x2: 1.1
Unemp.	36.8	5.2	57.8	41.2	6.2	52.5	
state at $t-5$							
↓							
Private	89.4	2.8	7.7	90.9	2.9	6.1	Max distance: 12.6
Public	20.9	76.4	2.6	18.3	78.4	3.1	Max distance, 2x2: 2.6
Unemp.	62.0	10.6	27.2	74.6	8.7	16.6	

Table 4.13: **Germany**: Fit to Job Mobility Data

	Observed			Predicted			
state at $t-1$	state at t			state at t			
↓	private	public	unemp.	private	public	unemp.	
Private	96.5	1.9	1.4	96.8	2.0	1.1	Max distance: 7.7
Public	7.8	90.8	1.3	6.5	92.6	0.7	Max distance, 2x2: 1.8
Unemp.	40.3	6.8	52.7	48.0	6.9	45.0	
state at $t-5$							
↓							
Private	94.2	3.2	2.4	94.2	4.0	1.6	Max distance: 6.5
Public	19.4	79.6	0.8	12.9	85.8	1.1	Max distance, 2x2: 6.5
Unemp.	79.0	6.4	14.5	81.5	8.9	9.5	

Table 4.14: **Netherlands**: Fit to Job Mobility Data

	Observed			Predicted			
state at $t-1$	state at t			state at t			
$\downarrow$	private	public	unemp.	private	public	unemp.	
Private	96.9	0.3	2.7	97.8	0.3	1.8	Max distance: 9.1
Public	0.8	98.2	0.8	0.6	98.7	0.5	Max distance, 2x2: 0.9
Unemp.	33.4	4.6	61.8	41.5	5.7	52.7	
state at $t-5$							
$\downarrow$							
Private	92.8	1.7	5.3	95.9	1.4	2.5	Max distance: 21.9
Public	5.4	93.0	1.4	4.2	95.1	0.6	Max distance, 2x2: 3.1
Unemp.	57.9	5.5	36.5	69.9	15.4	14.6	

Table 4.15: **France**: Fit to Job Mobility Data

	Observed			Predicted			
state at $t-1$	state at t			state at t			
$\downarrow$	private	public	unemp.	private	public	unemp.	
Private	95.2	1.9	2.7	96.0	1.8	2.1	Max distance: 2.9
Public	4.8	94.0	1.0	4.4	94.6	0.8	Max distance, 2x2: 0.8
Unemp.	21.9	4.7	73.3	24.1	5.3	70.4	
state at $t-5$							
$\downarrow$							
Private	90.3	6.6	2.9	91.5	6.3	2.0	Max distance: 3.6
Public	13.8	85.1	1.0	16.2	82.7	1.0	Max distance, 2x2: 2.4
Unemp.	51.5	12.9	35.5	53.4	14.6	31.9	

Table 4.16: **Italy**: Fit to Job Mobility Data

	Observed			Predicted			
state at $t-1$	state at t			state at t			
$\downarrow$	private	public	unemp.	private	public	unemp.	
Private	91.8	1.6	6.5	92.6	1.7	5.5	Max distance: 5.0
Public	7.4	89.9	2.5	6.6	91.4	1.9	Max distance, 2x2: 1.5
Unemp.	39.2	5.4	55.3	43.8	5.7	50.3	
state at $t-5$							
$\downarrow$							
Private	92.1	2.8	4.9	90.7	4.2	4.9	Max distance: 3.1
Public	18.0	80.5	1.3	15.1	83.6	1.1	Max distance, 2x2: 3.1
Unemp.	67.8	10.5	21.5	70.5	8.3	21.0	

Table 4.17: **Spain**: Fit to Job Mobility Data

	Observed			Predicted			
state at $t-1$	state at t			state at t			
$\downarrow$	private	public	unemp.	private	public	unemp.	
Private	94.8	2.5	2.6	95.2	2.4	2.2	Max distance: 6.1
Public	10.6	88.4	0.9	8.5	90.6	0.7	Max distance, 2x2: 2.2
Unemp.	35.8	7.7	56.4	41.5	8.0	50.3	
state at $t-5$							
$\downarrow$							
Private	91.5	4.2	4.1	91.5	6.0	2.4	Max distance: 7.5
Public	25.3	74.0	0.5	18.0	81.5	0.4	Max distance, 2x2: 7.5
Unemp.	69.9	12.5	17.4	72.9	16.3	10.6	

Table 4.18: **Portugal**: Fit to Job Mobility Data

	Observed					Predicted					
	earnings quintile at t					earnings quintile at t					
	75.0	19.5	3.5	1.5	0.4	73.7	19.7	4.8	1.4	0.1	
earnings	15.5	59.6	19.4	4.7	0.6	17.0	54.3	21.1	6.4	1.0	Max. dist.:
quintile	3.4	20.9	53.2	20.3	2.0	3.5	20.3	50.0	22.1	3.8	
at $t-1$	0.7	4.2	19.2	62.6	13.0	1.0	4.6	21.6	53.0	19.6	9.6
	0.1	0.9	1.7	13.4	83.7	0.1	0.6	3.5	18.5	77.1	
	62.1	19.7	10.1	5.9	1.9	56.0	24.5	9.5	6.4	3.3	
earnings	21.3	43.5	24.2	9.6	1.2	23.5	38.1	23.5	9.4	5.2	Max. dist.:
quintile	5.9	24.6	43.2	22.3	3.7	11.9	22.0	33.9	24.5	7.5	
at $t-5$	2.6	9.6	27.8	46.1	13.6	4.8	10.3	24.0	39.0	21.6	11.9
	0.2	1.3	2.7	21.8	73.7	2.9	4.5	9.8	20.8	61.8	

Note: earnings quintiles from the unconditional sample distribution.

Table 4.19: **Germany**: Fit to Wage Mobility Data

	Observed					Predicted					
	earnings quintile at t					earnings quintile at t					
	79.8	16.8	2.4	0.8	0.1	81.1	16.6	1.9	0.2	0.0	
earnings	15.7	65.4	16.0	2.4	0.3	15.4	62.2	19.8	2.3	0.1	Max. dist.:
quintile	1.9	16.1	64.3	16.0	1.5	1.6	19.1	57.9	19.9	1.3	
at $t-1$	0.4	1.8	16.7	70.0	10.8	0.1	2.1	18.9	63.5	15.2	6.5
	0.1	0.2	0.8	11.1	87.6	0.0	0.0	1.4	14.5	84.0	
	63.8	21.2	10.8	2.0	2.0	67.7	24.8	5.3	1.6	0.3	
earnings	26.1	45.0	20.3	7.2	1.0	21.4	44.4	24.4	7.3	2.1	Max. dist.:
quintile	3.8	31.8	43.0	16.0	5.2	6.5	21.0	41.9	24.5	5.8	
at $t-5$	1.6	4.6	24.5	53.3	15.8	0.8	7.4	23.0	47.9	20.8	10.8
	0.0	1.0	2.1	20.6	76.1	0.1	2.1	5.7	20.0	71.9	

Note: earnings quintiles from the unconditional sample distribution.

Table 4.20: **Netherlands**: Fit to Wage Mobility Data

	Observed					Predicted					
	earnings quintile at t					earnings quintile at t					
earnings quintile at $t-1$	75.3	20.8	3.2	0.3	0.1	78.0	18.5	2.6	0.6	0.0	Max. dist.:
	18.0	60.0	19.4	2.1	0.2	16.4	58.6	21.6	2.9	0.2	
	2.7	17.5	60.1	17.5	1.9	2.0	20.0	56.1	20.0	1.6	
	0.9	2.4	15.7	66.7	14.0	0.3	2.6	18.6	63.1	15.1	4.0
	0.2	0.9	1.6	12.8	84.3	0.0	0.0	1.4	14.3	84.1	
earnings quintile at $t-5$	67.1	24.2	6.3	2.2	0.0	63.5	24.9	7.8	3.3	0.3	Max. dist.:
	20.0	50.6	23.8	4.2	1.2	22.7	44.4	23.9	7.2	1.6	
	4.0	23.8	42.5	24.2	5.2	5.5	22.5	40.5	26.1	5.1	
	2.3	6.6	14.6	57.5	18.8	2.4	6.2	24.5	45.7	21.0	11.8
	0.8	2.2	3.5	16.2	77.0	0.8	1.6	4.3	19.3	73.7	

Note: earnings quintiles from the unconditional sample distribution.

Table 4.21: **France**: Fit to Wage Mobility Data

	Observed					Predicted					
	earnings quintile at t					earnings quintile at t					
earnings quintile at $t-1$	63.9	21.5	9.1	4.5	0.9	67.4	24.7	5.7	1.7	0.1	Max. dist.:
	22.5	45.9	21.6	7.9	1.9	22.3	45.1	24.7	6.3	1.3	
	7.8	22.8	45.2	19.2	4.7	5.0	23.0	43.4	23.4	5.0	
	2.1	7.1	24.1	50.1	16.4	1.0	5.8	22.9	48.4	21.6	5.2
	0.5	1.5	4.0	18.5	75.4	0.1	1.2	4.0	21.1	73.4	
earnings quintile at $t-5$	49.8	21.0	17.0	7.6	4.3	48.9	26.9	14.0	6.6	3.4	Max. dist.:
	30.0	29.2	24.2	12.1	4.2	24.4	30.8	26.9	12.2	5.4	
	12.4	27.1	33.3	17.1	9.9	10.9	24.8	28.3	23.3	12.4	
	6.6	10.0	23.0	38.6	21.5	6.4	10.2	22.7	33.6	26.9	6.6
	2.0	3.4	9.1	22.4	62.9	1.7	4.7	9.7	27.5	56.3	

Note: earnings quintiles from the unconditional sample distribution.

Table 4.22: **Italy**: Fit to Wage Mobility Data

	Observed					Predicted					
	earnings quintile at t					earnings quintile at t					
	59.2	28.5	10.1	1.8	0.2	67.4	25.0	6.3	1.0	0.0	
earnings	23.7	43.3	27.1	5.3	0.4	20.3	44.6	27.3	6.7	0.9	Max. dist.:
quintile	6.9	23.6	45.2	21.9	2.2	4.8	22.8	41.7	26.3	4.1	
at $t-1$	0.9	4.1	18.2	59.3	17.3	0.6	5.3	22.6	49.6	21.6	9.7
	0.1	0.3	1.8	16.4	81.1	0.0	0.6	3.2	19.2	76.8	
	47.6	32.4	12.9	6.0	0.9	47.2	31.1	14.6	5.9	1.0	
earnings	23.1	38.8	25.7	10.4	1.7	22.2	31.0	30.3	13.2	3.0	Max. dist.:
quintile	10.8	23.9	37.4	23.9	3.8	13.9	19.8	27.5	27.3	11.2	
at $t-5$	2.5	9.0	20.2	45.2	22.8	7.2	10.0	18.4	35.9	28.3	14.3
	0.0	1.4	1.8	19.7	76.9	2.7	3.6	8.5	22.3	62.6	

Note: earnings quintiles from the unconditional sample distribution.

Table 4.23: **Spain**: Fit to Wage Mobility Data

	Observed					Predicted					
	earnings quintile at t					earnings quintile at t					
	72.5	18.5	5.8	2.6	0.3	77.2	19.1	3.1	0.4	0.0	
earnings	16.0	60.1	17.2	5.5	0.9	17.3	56.3	20.7	5.2	0.2	Max. dist.:
quintile	4.5	19.3	57.3	17.3	1.4	2.9	19.4	55.8	19.6	2.0	
at $t-1$	1.4	2.9	16.1	69.3	10.0	0.5	4.3	18.7	61.6	14.7	7.7
	0.3	0.8	1.4	7.8	89.4	0.0	0.1	1.6	14.2	83.9	
	47.1	30.5	11.5	9.7	1.0	57.8	25.1	9.8	4.8	2.3	
earnings	31.4	32.7	17.6	15.9	2.1	23.0	37.9	25.3	10.8	2.8	Max. dist.:
quintile	12.0	30.6	31.3	19.3	6.5	11.3	22.7	35.8	24.6	5.3	
at $t-5$	3.6	7.6	25.1	48.5	14.9	5.7	11.8	22.1	40.8	19.4	11.7
	0.4	2.0	3.7	11.6	82.1	1.1	2.6	5.7	20.0	70.4	

Note: earnings quintiles from the unconditional sample distribution.

Table 4.24: **Portugal**: Fit to Wage Mobility Data

Sectoral composition of classes

	Observed				Predicted			
	% of sample	% private	% public	% unemp.	% of sample	% private	% public	% unemp.
$k^m = 1$	22.83	74.72	7.48	17.80	22.83	75.93	7.75	16.32
$k^m = 2$	54.84	95.81	0.00	4.19	54.84	95.72	0.00	4.28
$k^m = 3$	22.33	15.07	82.94	2.00	22.33	14.33	84.30	1.37
	100.00				100.00			

Class composition of the sectors

	Observed				Predicted			
	% of sample	% $k^m = 1$	% $k^m = 2$	% $k^m = 3$	% of sample	% $k^m = 1$	% $k^m = 2$	% $k^m = 3$
Private	72.97	23.38	72.01	4.61	73.03	23.74	71.88	4.38
Public	20.22	8.44	0.00	91.56	20.59	8.59	0.00	91.41
Unemp.	6.81	59.70	33.75	6.55	6.38	58.42	36.78	4.81
	100.00				100.00			

Table 4.25: **Germany**: Mobility Class and Sector composition, Real and Simulated

Mobility classes						
	% high ed.	% med. ed.	% low ed.	Mean exp.	$\%k^y = 1$	$\%k^y = 2$
$k^m = 1$	14.76	68.16	17.08	20.45	29.27	70.73
$k^m = 2$	28.36	59.53	12.11	16.63	59.10	40.90
$k^m = 3$	37.20	59.24	3.55	17.99	60.05	39.95
Total	27.23	61.43	11.34	17.81	52.50	47.50

Wage classes							
	% high ed.	% med. ed.	% low ed.	Mean exp.	% $k^m = 1$	% $k^m = 2$	% $k^m = 3$
$k^y = 1$	25.39	59.26	15.35	18.03	12.73	61.74	25.54
$k^y = 2$	29.27	63.83	6.90	17.56	34.00	47.22	18.78
Total	27.23	61.43	11.34	17.81	22.83	54.84	22.33

Table 4.26: **Germany**: Composition and Joint Distribution of Unobserved Heterogeneity Classes

	$k^y = 1$	$k^y = 2$
% of sample	52.50	47.50
Mean private (log) wage	8.46	8.44
Std. dev., private wage	0.31	0.39
Mean public (log)wage	8.45	8.44
Std. dev., public wage	0.30	0.38

Table 4.27: **Germany**: Wage Classes Mean Earnings

Sectoral composition of classes

	Observed				Predicted			
	% of sample	% private	% public	% unemp.	% of sample	% private	% public	% unemp.
$k^m = 1$	18.41	0.00	99.39	0.61	18.41	0.00	98.77	1.23
$k^m = 2$	17.99	63.14	28.65	8.21	17.99	63.90	28.32	7.78
$k^m = 3$	63.60	98.31	0.00	1.69	63.60	98.59	0.00	1.41
	100.00				100.00			

Class composition of the sectors

	Observed				Predicted			
	% of sample	% $k^m = 1$	% $k^m = 2$	% $k^m = 3$	% of sample	% $k^m = 1$	% $k^m = 2$	% $k^m = 3$
Private	73.88	0.00	15.38	84.62	74.20	0.00	15.50	84.50
Public	23.45	78.02	21.98	0.00	23.28	78.11	21.89	0.00
Unemp.	2.67	4.23	55.39	40.37	2.52	8.96	55.53	35.51
	100.00				100.00			

Table 4.28: **Netherlands**: Mobility Class and Sector composition, Real and Simulated

Mobility classes	% high ed.	% med. ed.	% low ed.	Mean exp.	$\%k^y = 1$	$\%k^y = 2$
$k^m = 1$	43.21	43.86	12.93	21.76	76.63	23.37
$k^m = 2$	32.67	43.88	23.46	15.27	46.73	53.27
$k^m = 3$	17.26	61.57	21.17	17.98	46.08	53.92
Total	24.81	55.13	20.06	18.19	51.82	48.18

Wage classes	% high ed.	% med. ed.	% low ed.	Mean exp.	% $k^m = 1$	% $k^m = 2$	% $k^m = 3$
$k^y = 1$	25.51	45.30	29.18	18.31	27.22	16.23	56.55
$k^y = 2$	24.05	65.70	10.25	18.06	8.93	19.89	71.17
Total	24.81	55.13	20.06	18.19	18.41	17.99	63.60

Table 4.29: **Netherlands**: Composition and Joint Distribution of Unobserved Heterogeneity Classes

	$k^y = 1$	$k^y = 2$
% of sample	51.82	48.18
Mean private (log) wage	8.70	8.69
Std. dev., private wage	0.35	0.32
Mean public (log)wage	8.61	8.73
Std. dev., public wage	0.33	0.29

Table 4.30: **Netherlands**: Wage Classes Mean Earnings

Sectoral composition of classes

	Observed				Predicted			
	% of sample	% private	% public	% unemp.	% of sample	% private	% public	% unemp.
$k^m = 1$	14.26	39.79	16.38	43.83	14.26	39.70	15.75	44.55
$k^m = 2$	32.86	42.02	57.67	0.31	32.86	42.46	57.27	0.27
$k^m = 3$	52.87	92.91	5.09	1.99	52.87	93.50	4.53	1.96
	100.00				100.00			

Class composition of the sectors

	Observed				Predicted			
	% of sample	% $k^m = 1$	% $k^m = 2$	% $k^m = 3$	% of sample	% $k^m = 1$	% $k^m = 2$	% $k^m = 3$
Private	68.61	8.27	20.13	71.60	69.06	8.20	20.21	71.59
Public	23.98	9.74	79.03	11.23	23.47	9.58	80.21	10.21
Unemp.	7.41	84.38	1.39	14.23	7.48	84.95	1.17	13.87
	100.00				100.00			

Table 4.31: **France**: Mobility Class and Sector composition, Real and Simulated

Mobility classes	% high ed.	% med. ed.	% low ed.	Mean exp.	$\%k^y = 1$	$\%k^y = 2$	
$k^m = 1$	30.65	34.99	34.36	7.91	38.85	61.15	
$k^m = 2$	17.18	46.46	36.36	24.20	48.44	51.56	
$k^m = 3$	29.89	48.08	22.03	17.00	50.66	49.34	
Total	25.82	45.68	28.50	18.07	48.25	51.75	
Wage classes							
	% high ed.	% med. ed.	% low ed.	Mean exp.	% $k^m = 1$	% $k^m = 2$	% $k^m = 3$
$k^y = 1$	30.93	30.60	38.47	18.46	11.48	33.00	55.52
$k^y = 2$	21.06	59.75	19.19	17.70	16.85	32.74	50.41
Total	25.82	45.68	28.50	18.07	14.26	32.86	52.87

Table 4.32: **France**: Composition and Joint Distribution of Unobserved Heterogeneity Classes

	$k^y = 1$	$k^y = 2$
% of sample	48.25	51.75
Mean private (log) wage	9.40	9.39
Std. dev., private wage	0.45	0.41
Mean public (log)wage	9.47	9.38
Std. dev., public wage	0.43	0.40

Table 4.33: **France**: Wage Classes Mean Earnings

Sectoral composition of classes

	Observed				Predicted			
	% of sample	% private	% public	% unemp.	% of sample	% private	% public	% unemp.
$k^m = 1$	49.08	89.49	6.41	4.11	49.08	89.88	6.33	3.79
$k^m = 2$	34.76	24.18	58.24	17.58	34.76	24.83	57.84	17.33
$k^m = 3$	16.16	29.26	27.54	43.20	16.16	30.04	28.38	41.58
	100.00				100.00			

Class composition of the sectors

	Observed				Predicted			
	% of sample	% $k^m = 1$	% $k^m = 2$	% $k^m = 3$	% of sample	% $k^m = 1$	% $k^m = 2$	% $k^m = 3$
Private	57.06	76.98	14.73	8.28	57.60	76.59	14.98	8.43
Public	27.84	11.29	72.72	15.99	27.80	11.18	72.33	16.49
Unemp.	15.11	13.34	40.45	46.20	14.60	12.75	41.25	46.00
	100.00				100.00			

Table 4.34: **Italy**: Mobility Class and Sector composition, Real and Simulated

Mobility classes						
	% high ed.	% med. ed.	% low ed.	Mean exp.	$\%k^y = 1$	$\%k^y = 2$
$k^m = 1$	7.16	55.92	36.91	17.99	43.59	56.41
$k^m = 2$	12.11	51.69	36.20	16.04	32.62	67.38
$k^m = 3$	8.16	38.03	53.81	19.14	67.43	32.57
Total	9.04	51.56	39.40	17.50	43.63	56.37

Wage classes							
	% high ed.	% med. ed.	% low ed.	Mean exp.	% $k^m = 1$	% $k^m = 2$	% $k^m = 3$
$k^y = 1$	13.88	53.47	32.65	20.58	49.04	25.99	24.97
$k^y = 2$	5.30	50.08	44.62	15.11	49.12	41.55	9.33
Total	9.04	51.56	39.40	17.50	49.08	34.76	16.16

Table 4.35: **Italy**: Composition and Joint Distribution of Unobserved Heterogeneity Classes

	$k^y = 1$	$k^y = 2$
% of sample	43.63	56.37
Mean private (log) wage	8.01	7.86
Std. dev., private wage	0.34	0.26
Mean public (log)wage	8.03	7.88
Std. dev., public wage	0.33	0.24

Table 4.36: **Italy**: Wage Classes Mean Earnings

Sectoral composition of classes

	Observed				Predicted			
	% of sample	% private	% public	% unemp.	% of sample	% private	% public	% unemp.
$k^m = 1$	20.03	3.37	93.39	3.23	20.03	3.36	92.93	3.72
$k^m = 2$	21.62	47.84	4.46	47.69	21.62	47.18	5.22	47.60
$k^m = 3$	58.35	87.79	0.53	11.68	58.35	87.09	0.56	12.35
	100.00				100.00			

Class composition of the sectors

	Observed				Predicted			
	% of sample	% $k^m = 1$	% $k^m = 2$	% $k^m = 3$	% of sample	% $k^m = 1$	% $k^m = 2$	% $k^m = 3$
Private	62.24	1.09	16.62	82.30	61.69	1.09	16.53	82.38
Public	19.98	93.61	4.83	1.56	20.07	92.76	5.62	1.62
Unemp.	17.77	3.64	58.01	38.35	18.24	4.08	56.40	39.52
	100.00				100.00			

Table 4.37: **Spain**: Mobility Class and Sector composition, Real and Simulated

Mobility classes							
	% high ed.	% med. ed.	% low ed.	Mean exp.	$\%k^y = 1$	$\%k^y = 2$	
$k^m = 1$	58.50	23.57	17.92	19.53	69.64	30.36	
$k^m = 2$	25.23	18.38	56.40	21.90	0.00	100.00	
$k^m = 3$	28.09	24.58	47.33	16.19	50.32	49.68	
Total	33.56	23.04	43.40	18.09	43.31	56.69	
Wage classes							
	% high ed.	% med. ed.	% low ed.	Mean exp.	$\%\ k^m = 1$	$\%\ k^m = 2$	$\%\ k^m = 3$
$k^y = 1$	40.71	26.04	33.25	20.30	32.21	0.00	67.79
$k^y = 2$	28.10	20.74	51.15	16.41	10.73	38.13	51.14
Total	33.56	23.04	43.40	18.09	20.03	21.62	58.35

Table 4.38: **Spain**: Composition and Joint Distribution of Unobserved Heterogeneity Classes

	$k^y = 1$	$k^y = 2$
% of sample	43.31	56.69
Mean private (log) wage	12.33	12.16
Std. dev., private wage	0.45	0.40
Mean public (log)wage	12.42	12.29
Std. dev., public wage	0.41	0.38

Table 4.39: **Spain**: Wage Classes Mean Earnings

Sectoral composition of classes

	Observed				Predicted			
	% of sample	% private	% public	% unemp.	% of sample	% private	% public	% unemp.
$k^m = 1$	25.45	17.14	74.68	8.18	25.45	14.11	77.29	8.60
$k^m = 2$	62.43	94.45	0.00	5.55	62.43	94.26	0.00	5.74
$k^m = 3$	12.11	68.93	7.29	23.77	12.11	70.39	5.88	23.73
	100.00				100.00			

Class composition of the sectors

	Observed				Predicted			
	% of sample	% $k^m = 1$	% $k^m = 2$	% $k^m = 3$	% of sample	% $k^m = 1$	% $k^m = 2$	% $k^m = 3$
Private	71.68	6.09	82.27	11.65	70.97	5.06	82.93	12.01
Public	19.89	95.56	0.00	4.44	20.39	96.51	0.00	3.49
Unemp.	8.43	24.71	41.13	34.16	8.65	25.31	41.45	33.24
	100.00				100.00			

Table 4.40: **Portugal**: Mobility Class and Sector composition, Real and Simulated

Mobility classes						
	% high ed.	% med. ed.	% low ed.	Mean exp.	$\%k^y = 1$	$\%k^y = 2$
$k^m = 1$	16.03	20.78	63.18	17.80	46.49	53.51
$k^m = 2$	3.52	14.73	81.75	15.81	50.28	49.72
$k^m = 3$	12.25	9.26	78.49	25.33	20.25	79.75
Total	7.76	15.61	76.63	17.47	45.68	54.32

Wage classes							
	% high ed.	% med. ed.	% low ed.	Mean exp.	% $k^m = 1$	% $k^m = 2$	% $k^m = 3$
$k^y = 1$	5.96	16.48	77.55	16.35	25.91	68.72	5.37
$k^y = 2$	9.27	14.88	75.85	18.41	25.08	57.15	17.78
Total	7.76	15.61	76.63	17.47	25.45	62.43	12.11

Table 4.41: **Portugal**: Composition and Joint Distribution of Unobserved Heterogeneity Classes

	$k^y = 1$	$k^y = 2$
% of sample	45.68	54.32
Mean private (log) wage	11.70	11.79
Std. dev., private wage	0.38	0.45
Mean public (log)wage	11.81	11.84
Std. dev., public wage	0.45	0.53

Table 4.42: **Portugal**: Wage Classes Mean Earnings

Chapter 5

Concluding Comments

In this thesis I have addressed three questions concerned with the formation of human capital and how it is rewarded in the labour market. Chapters 2 and 3 are related by my interest in how education affects individual's outcomes, both in the short and longer-term, and the implications this has for policy. Chapter 4 addresses another related question in labour economics, concerning the existence of inequalities between the public and private sectors of the labour market in how individuals are remunerated over a working lifetime.

My aim in Chapter 2 was to evaluate whether the policy of introducing free early education places for all 3-year olds has had any impact on test results when children reach age seven, and moreover whether there has been any additional effect in the geographical areas deemed most in need of free early education places.

The policy evaluation regressions revealed that it is only in the poorer LEAs that there are identifiable policy effects – so the areas that the Government targeted ***were*** affected by the policy's introduction. In terms of *how* the policy affected results, though the policy increased provision for 3-year olds in the non-schools sector – comprised largely of private sector places – there were no measured robust effects of this increase on results in reading or writing either overall or for either the more or less deprived LEAs when estimated separately.

My main finding is that in the poorer LEAs, there is a small but significant and robust positive effect of take-up by 3-year olds of free early education places in maintained nursery and primary schools, on the percentage of children attaining at least the expected level in Key Stage 1 reading and writing. The effects are quite small in magnitude: a 10%-point increase in take-up leading to an improvement in results of approximately one-third of a within standard deviation. However, these effects are comparable in size to reducing average KS1 class size by almost one child in the case of reading, and three-quarters of a child in the case of writing. My findings are in line with the EPPE study which suggests that children attending maintained settings for pre-school perform better on school entry and at age 7.

There is also tentative evidence that in the better-off LEAs there is a significant and robust positive effect of the take-up by 4-year olds of free early education places in maintained nursery and primary schools, on the percentage of children attaining a higher than expected level in KS1 reading, writing and maths. The effects are still small in absolute terms but represent increases of three-quarters of a within standard deviation (reading), half of a within standard deviation (writing) and one third of a within standard deviation (maths), for a 10%-point increase in take-up. As the majority of 4-year olds attending a free early education place in a maintained nursery or primary school attend a reception or infant class in a primary school, these results indicate that, in the better off LEAs, increasing the proportion of children who start primary school early has a positive effect on KS1 results increasing the percentage of children who exceed expected levels of attainment. Nevertheless this is a cautious conclusion due to concerns over an upward bias in the estimated coefficient.

With respect to policy, the conclusion is clear that it is increased take-up in the maintained nursery and primary schools that are associated with improved test performance. This therefore suggests that expansion of the capacity of these providers, rather than channelling government funding to private providers, is the way to improve attainment – giving a clear lead for early years education policy. The magnitude of the improvements in educational outcomes suggests that results improvement alone may not necessarily be a justification for such universal provision. However, the externalities associated with early education, including the behavioural and socialisation gains and the development of non-cognitive skills which then facilitate and enhance learning, also need to be considered. In addition, this policy works in conjunction with other policies which aim to improve the labour market attachment of parents, particularly single parents, and there are wider benefits to children, parents and society as a whole when parents are helped back into work.

In the third chapter, I assess the extent to which different instrumental variable estimates of the return to education capture different 'local average treatment effects' and which is most appropriate for making inference about *the* return to education in Britain. I present three IV estimates: using RoSLA (10.2%), both RoSLA and early smoking (12.5%), and early smoking alone (12.9%). All of which are substantially higher than the OLS estimate of 4.6%. Moreover, these IV estimates whilst being statistically significant are sufficiently imprecise for me to be unable to conclude are actually different from each other. My analysis suggests that the RoSLA estimate captures the return for the individuals who wanted to leave at the minimum leaving age but were forced to stay longer, whereas I have argued that early smoking is a behaviour engaged in by individuals of all abilities who have high discount rates due to their rate of time preference. I conclude that the IV estimate derived from the early smoking instrument is closer to an average marginal return to education, purged of the bias of OLS. That both the RoSLA and early smoking IV estimates are not statistically different to each other suggests that the RoSLA LATE is also close to an average marginal return to education and therefore that the returns at the lower part of the distribution are similar to the average return.

For future work, it is important to recognize that there are issues surrounding the appropriateness of the linearity assumption and the reality of heterogeneous returns to education across individuals when modelling the return to education – indeed it may not even be appropriate to refer to ***the*** causal effect of education on earnings.

At the same time, from a policy perspective it has arguably never been more important to understand and have an accurate assessment of how education affects wages, as the Government introduces changes to the minimum education leaving age: raising it first to 17 (2013) and then to 18 (2015). Moreover, linking to my fourth chapter, it is important to evaluate the effect of education not just on instantaneous earnings but on the employment and earnings dynamics over a lifetime. This is an area of research to which I am keen to contribute, modelling the effects of education over a lifetime.

My final substantive chapter looks at public-private pay differences in Germany, the Netherlands, France, Italy, Spain and Portugal. Rather than concentrating on cross-sectional differences in earnings levels, the chapter applies a flexible model of earnings and employment dynamics, where the individual earnings and employment trajectories are conditioned by unobserved as well as observed individual heterogeneity. The present discounted values of lifetime earnings flows for each individual in each sector are compared. This allows a more complete picture of inequalities between the sectors, taking account of the fact that forward looking individuals care not only about earnings levels but also earnings and job mobility, which also differ markedly between the sectors.

For each country the model does a good job of fitting the observed cross-sectional distribution of workers into sectors and the cross sectional earnings distributions, and importantly also the patterns of labour market mobility and earnings mobility.

The most important finding is that when we simulate movement between employment sectors in addition to earnings trajectories, the average public premium in lifetime values is approximately zero for Germany, the Netherlands, Spain and Portugal, whereas in Italy and France there is a small positive public premium. The conclusion we draw from this is that in each country there is sufficient job mobility, that individuals are quickly sorted into their 'natural' sector.

We also find that in most of the countries, the public premium in lifetime values remains uniform and close to zero as we move up the percentiles of the distribution. This suggests that where we do observe somewhat greater earnings compression in the public sector, it is due to a lower variance in the transitory component of earnings in the public sector.

The conclusions drawn from this chapter suggest that inequality between the public and private sectors in terms of earnings levels and compression are largely reflections of the different human capital and preferences of the labour force in each sector. For most of the countries in the study, the labour market is sufficiently mobile to allocate workers to their 'natural' sector relatively quickly.

For future work, this methodology can be adapted to look at the inequality in outcomes between other socio-economic groups over time. This could be applied to looking at difference in gender pay, or differences in pay related to broad educational groups, as alluded to above. Each of these are likely to be fruitful areas of future research.

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Appendix A

Appendices to Chapter 2

A.1 Data Appendix

A.1.1 Sources of Data

The figures that I have for the number of 3 year olds and 4 year olds taking free early education places are collected from different sources before being published in the DfES reports that I take them from. Since 1996, the Annual Schools Census has collected data on 3 and 4-year olds in schools each January. For each age, 3 and 4, the ASC records the number of children of that age at 31st December in the previous year attending nursery or primary school part-time or full-time. All such children are counted as receiving a free early education place.

Information on the number of 3 and 4-year olds taking up free places in the 'other' sector comprising other maintained settings, private, voluntary and independent schools is derived from the Nursery Education Grant Data Collection Exercise. This NEG data collection exercise has been carried out by the DfES annually since January 1999, and for each age, 3 and 4, records the part-time equivalent number of children of that age at 31st December the previous year, receiving the free early education place.

The two main DfES publications I use are:

"Statistics of Education: Provision for children under five years of age in England - January 2002", National Statistics Bulletin for the DfES, Issue No. 08/02, December 2002.

"Provision for children under five years of age in England: January 2004 (Final)", National Statistics Statistical First Release for the DfES, SFR 39/2004, October 2004.

A child attending maintained nursery school or a nursery or infant class in a maintained primary school is counted as taking a free early education place, according to these DfES publications. In some cases the information recorded in the annually reported publications of pupils under 5-years of age in maintained schools in England is not rounded whereas the free early education places data in the above publications are rounded. This is the case for 1998 and 1999 data for 3-year olds and the 1998, 1999 and 2000 data for 4-year olds. Therefore in these cases I use the annual publications for the number of children in maintained nursery and primary schools. In each case it is the Annual Schools Census that the data comes from, the only difference is whether it is rounded or not. The annual publications used therefore are:

"Pupils Under Five Years of Age in Schools in England - January 1998", Statistical Bulletin 10/98, October 1998.

"Pupils Under Five Years of Age in Maintained Schools in England - January 1999". Statistical First Release for the DfES, 8/1999, May 1999.

"Statistics of Education: Provision for children under five years of age in England - January 2000", National Statistics Bulletin for the DfES, Issue No. 01/01, January 2001.

The headcount data from the Annual Schools Census tells us how many children use places – it is the number of children receiving at least one funded session, which means attending nursery/primary school at least once per week part-time; while the part-time equivalent places data tells us how many half-day places are funded. These two measures are not totally equivalent. However, at the national level, there is the data on the number of children who receive early education funded by the NEG – so a measure equivalent to the headcount measure. When this is compared to the part-time equivalent number of places funded the discrepancy is small in every year bar 2003, which suggests that it is reasonable to treat the two sector measurements as comparable. Indeed this is explicit in the discussion of trends in the DfES publications and is the line taken by Brewer *et al.* (2005) when analysing differential take-up of places in each sector, by deprivation level. For 2003, the discrepancy is larger, the difference between the number of 3-year old children receiving at least one funded session and the PTE number of places being approximately 18% of the number of children receiving at least one funded session. However, I robustness check my results by excluding 2003 data for 3-year olds when I exclude the 2006 results data, and almost all of the results remain.

The comparability of the nursery and primary school attendee data and the NEG PTE number of places in the 'other' sector relies on either (a) the PTE number of places being approximately equal to the number of children who receive at least one session - i.e. the assumption is that children use their full entitlement; or (b) the school/nursery attendees are using their full entitlement i.e. attending nursery/primary school part-time at least. The figures nationally for the comparison of PTE places in the 'other' sector and children receiving at least one funded session in the other sector show that for all years (bar 2003), the assumption (a) holds at the national level which is an encouraging indication that it is fair to make this assumption. If we assume that children attending a nursery or primary school for their early education do attend part-time (i.e. take up their 5-session allocation) then we do not have to worry about assumption (a) because if all children attending nursery/primary school attend part-time then this tells us the PTE number of places in this sector and is directly comparable with the 'other' sector figure that we have. Given that both (a) and (b) appear to be close approximations to reality, it is fair to assume, as the Government and other authors do, that we can directly compare the numbers in each sector. It is worth noting that if a child takes a place in a maintained nursery/primary school for at least part of their free early education, it is not possible to then redeem other sessions at a private provider – which makes it even more likely that children attending nursery and primary schools do so at least for their 5 sessions otherwise they lose their entitlement.

The fact that 3- and 4-year old children attending a maintained nursery or primary school part-time are counted as having a free early education place is made explicit in the publication: "Statistics of Education: Provision for children under five years of age in England - January 2002", National Statistics Bulletin for the DfES, Issue No. 08/02, December 2002: paragraph 19, p. 6.

Part-time in nursery or primary school means that the child does not attend both morning and afternoon for 5 days per week, this is outlined in "Pupils Under Five Years of Age in Schools in England - January 1998" (Statistical Bulletin Number 10/98). We do not know whether the children attend half day for 5 days a week, but given that they do attend either part-time or full-time, we only need the average to be half day for 5 days a week to make it a comparable measure to the PTE that we have in the 'other' sector.

It is important to recognise that there are problems with each measure – both the maintained schools sector measure and the 'other' sector measure – but that they are approximately equal and comparable. Indeed this is implicitly assumed in both the Government and other authors' analysis of the data.

A.1.2 Variables Construction

For the main variables of interest – the take-up rate of places in each sector by 3-year olds – the data is explicitly recorded for each year without exception. For 4-year olds in the years 2002 and 2004, this is also the case (it is the case also in 2003 but, aside from the robustness checks, 2003 is not used for 4-year olds data as this corresponds to the year 2002 data that is missing for 3-year olds).

However, for the years 1999, 2000 and 2001, the reported NEG data for 4-year olds comprises the part-time equivalent number of funded places in the 'other' sector **plus** the number of 4-year olds attending a nursery or primary school. Therefore using the ASC data on the number of 4-year olds attending nursery or primary schools and the total from the NEG data, it is possible to estimate the number of 4-year olds receiving their free early education place in the 'other' sector, which is what I do. As mentioned, for the years 2002 and 2004, the figures are explicitly separated for 4-year olds in the 2004 DfES publication thus we do not have to estimate them. Within each LEA, comparing the number of children taking a place in each sector for 4-year olds in 2002 and 2004 when these are reported explicitly, with the numbers for 1999, 2000 and 2001 which I calculate, the figures are very close. This is true in terms of actual numbers and in relative percentages. Therefore I am confident that this approach to estimating the number of places taken by 4-year olds in the 'other' sector for the first three years, is valid.

When children are 4 years old, it is much more likely that they are indeed attending nursery or primary school at least part-time, and the numbers that I have on the number of 4-year olds in nursery and primary school count each child if they are attending part-time **or** full-time (there is no double counting of full-time children). If we make the assumption that 4-year olds are attending at least part-time and having that part-time place funded, then when we take the number of 4-year olds attending nursery or primary schools from the NEG total, we will be left with the number of part-time NEG funded places in the 'other' sector. The school measure and the total NEG measure will be capturing the same thing, attendance part-time in early education. Therefore we do not need to worry that one is measuring at least one session and the other is measuring part-time attendance (i.e. taking all of 5 sessions or being included as 0.6 if taking only 3) as both will be measuring the number of children attending part-time.

There are a small number of cases (22 out of the 575 main sample observations) in which the total number of funded places recorded in the NEG data for 4-year olds when compared with the nursery and primary school attendance data for 4-year olds implies a negative number of places in the 'other' sector. In these cases, the 'other' sector places were set to zero. This will only be the case if we do have some 4-year olds who are not attending school part-time, in which case the number receiving at least one session in school will be more than the number of funded places in school leading to this negative when the school attendees are taken from the NEG total. As noted, this is only the case in 22 of 575 observations.

A.1.3 Childcare Availability Variables

The figures on availability of places in day nursery, playgroup or with childminders refer to places available for children under 8-years old. However, it is fair to assume that the majority of provision is utilised by children of pre-compulsory school age. Playgroups in particular are almost exclusively for the use of children aged 3 and 4, therefore I formalise this as an assumption and use the information on population aged 3 and 4 in the LEA and the total number of playgroup places to calculate the number of playgroup places available per 100 children aged 3 and 4. As day nursery and childminder places can be used by children of all ages from birth to 4, I assume that the proportion allocated to children age 3 or 4 is equal to the population proportion aged 3 or 4 and calculate the number of places per 100 children aged 3 or 4 on that basis.

A.1.4 Example of how birth month affects school start date

As set out in the data section, the birth month of a child within a year determines the exact time that they are able to start school – for children born in the same calendar year there are two possible school year groups that they could be in. For example, for children born in the calendar year 1994, all of whom would be recorded as 3-year olds in the **January 1998** early education place data, the situation is as follows:

Date of Birth	Turn 3	Turn 4	Turn 5
1st Jan 1994 to 31st March 1994	1st Jan 1997 to 31st March 1997 Entitled to a free place from 1st April 1997	1st Jan 1998 to 31st March 1998 Still in free early education place	1st Jan 1999 to 31st March 1999 Start Reception in Sept 1998 or Jan 1999 therefore finish R in July 1999 and finish Y2 in July 2001.
1st April 1994 to 31st Aug 1994	1st April 1997 to 31st Aug 1997 Entitled to a free place from 1st Sep 1997	1st April 1998 to 31st Aug 1998 Still in free early education place	1st April 1999 to 31st Aug 1999 Start Reception in April 1999 or Jan 1999 or Sept 1998 therefore finish R in July 1999 and finish Y2 in July 2001.
1st Sep 1994 to 31st Dec 1994	1st Sep 1997 to 31st Dec 1997 Entitled to a free place from 1st Jan 1998	1st Sep 1998 to 31st Dec 1998 Still in free early education place	1st Sep 1999 to 31st Dec 1999 Start Reception in Sept 1999 therefore finish R in July 2000 and finish Y2 in July 2002.

Therefore irrespective of whether the LEA's policy is to start children in the academic term or academic year that they turn 5 years old, the children born in the first eight months of the year will be in one year group, taking their KS1 assessments 3 years after they are recorded as 3-year olds, with the children born later in the year taking their KS1 assessments 4 years after they are recorded as 3-year olds. The key thing is that terms in reception class adjust, such that all children start year 1 in September when they are age 5.

A.1.5 Samples

The main estimation sample consists of 120 of the 150 LEAs in England. Of these 120, 115 were in existence with entirely the same boundaries and jurisdiction for the entire period from 1998-2003. The remaining 5 LEAs[1] were in existence for the entire time period but had their boundaries changed between the 1998 data and subsequent years. For these latter 5 LEAs, though their boundaries are changed, their early education place data and population data refer to the correct geographical areas so this is not a problem.

The second estimation sample includes an additional 10 LEAs who were either boundary changing LEAs[2] or new LEAs in 1998[3]. The seven LEAs that are new in 1998, have free early education place take-up data – applied via apportionment by the DfES – and population data, however they do not have any childcare availability data for 1998. Rather than making

[1] Cambridgeshire, Kent, Lancashire, Nottinghamshire and Shropshire.

[2] Cheshire, Devon and Essex.

[3] Blackburn with Darwen, Blackpool, Medway, Nottingham (City), Peterborough (City), Telford and Wrekin, Thurrock.

assumptions about these LEAs in 1998 and mapping data to them from their 'parent' LEAs, I exclude the 1998 observation of these seven new LEAs. Similarly, the 1998 observations of the three additional boundary changer LEAs added in this sample are excluded due to differences in the geographical areas that their early education variables and childcare availability variables refer to in 1998.

Therefore, though the second sample adds 10 LEAs, for each of them their 1998 observation is excluded. This obviously unbalances the panel somewhat hence the exclusion of these LEAs from the main sample.

As with the main estimation sample, in the second sample, each LEA again contributes between 3 and 5 observations with a mean of 4.72 observations per LEA for the level 2B or higher analysis, and a mean of 3.87 observations per LEA for the level 3 or higher analysis. In this second sample, three of the additional 10 LEAs are from the pathfinder group, so they comprise 60 of 130 LEAs in this sample, which is 46% of the sample of LEAs.

There are 2 LEAs (City of London and Isles of Scilly) that are never used because they never have sufficient data. The remaining 18 that are not in the main or second samples are excluded because they have missing data in one or more years and thus cannot provide 5 observations to keep the panel largely balanced.

The final sample (3) used is the "all observations" sample, in which any observations from an LEA that has all the necessary variables for the regression is included, though continuing to exclude the 1998 observations of the 10 LEAs introduced in the second sample. This means that information from 148 of the 150 LEAs is used with each contributing between 2 and 5 observations, with an average of 4.59 observations per LEA for the level 2B or higher analysis, and 3.82 observations per LEA for the level 3 or higher analysis. Clearly the panel is unbalanced in this case, and potentially biased by differential attrition, however as a robustness check of the results this sample is included. With 148 LEAs included, all 65 of the group first to receive the NEG for 3-year olds are included, making up 44% of the sample of LEAs.

The only problem for the 5 LEAs who are part of the main estimation sample but do have their boundaries changed between the 1998 and 1999 data (Cambridgeshire, Kent, Lancashire, Nottinghamshire and Shropshire) is that for 1998 their childcare availability variables refer to the LEA before the boundaries were re-drawn. However, they are included in the main sample because I have additional population data from 1998 for the geographical areas that these childcare variables pertain to, so that their childcare availability rate variables can be created with the population in the denominator referring to the correct geographical area. Moreover, as the variables are all rates, I consider them to have sufficient reliability to be included in the main sample. This additional population data that I have for the population aged 3 and 4 and the population aged under-5 calculated from the contemporary nursery attendance publication ("Pupils Under Five Years of Age in Schools in England – January 1998", Statistical Bulletin 10/98, October 1998) and the contemporary day care places publication ("Statistics of Education: Children's Day Care Facilities at 31 March 1998 England", DfES, 1999). These publications provided information for the pre-boundary change areas, hence the population figures in these publications refer to the same geographical areas as the childcare variables. For the free early education places take-up, the geographical area referred to is the post-boundary change area as are the National Statistics population estimates, hence there is no problem with the creation of these variables.

For all other LEAs there was no change in boundaries for any time during the panel, therefore their childcare data as well as their early education place data refers to the same geographical area as do the National Statistics population estimates, hence there is never a problem with the variables creation for these LEAs.

The problem for the three boundary changers that are not in the main sample and have their 1998 observation excluded when they are used in the second sample is that the contemporary nursery attendance and day care place availability publications do not refer to the same geographical areas as each other thus the populations estimated as age 3 and 4, and under 5 are not for comparable areas. The nursery data that could be used to estimate the population aged 3 and 4 refers to the pre-change boundaries, whereas the day care places data refers to the boundaries post-change. For this reason we have to exclude these LEAs from the main sample and exclude their 1998 observation when they are used in the second sample.

The problem for the seven 'new' LEAs (Blackburn with Darwen, Blackpool, Medway, Nottingham (City), Peterborough (City), Telford and Wrekin, Thurrock) who are included in the second sample, though with their 1998 observation excluded, is that they do not have any childcare data available in 1998. These LEAs have a parent LEA from whom they could have data mapped to them for 1998, however this creates additional problems of the applicability of this data and so it was decided to exclude their 1998 observation and only include them in the second sample.

There are a number of other LEAs that are newly created in 1998 but whose parent LEA does not have data for 1998 to map to them, hence their exclusion. In theory they have no less data than the 7 who are included in sample 2 but to include them all would seriously unbalance the panel and we always have the option of including the 7 LEAs with mapped data.

Of the 19 newly created LEAs in 1998, all have free early education place data but differ in their childcare data availability: Six have childcare data and are therefore included in the main estimation sample (sample 1): Halton, Plymouth (City), Southend, Torbay, West Berkshire, Wokingham.

Seven do not have the childcare data but could potentially have data mapped to them: Blackburn with Darwen, Blackpool, Medway, Nottingham (City), Peterborough (City), Telford and Wrekin, Thurrock.

One is excluded because of later missing data: Warrington.

Five do not have childcare data and there is not a parent LEA with data to map to them: Bracknell Forest, Reading, Slough, Windsor and Maidenhead, Herefordshire.

Tables A.1 and A.2 list the LEAs in each sample; Table A.3 lists the LEAs that were in the 'pathfinder' group of poorer LEAs who were first to receive the Nursery Education Grant for 3-year olds.

Table A.1: Local Education Authorities: in Main Sample

Barking and Dagenham	Hertfordshire	Rochdale
Barnet	Hillingdon	Rutland
Barnsley	Hounslow	Salford
Bath and NE Somerset	Isle of Wight	Sandwell
Bedfordshire	Islington	Sefton
Bexley	Kensington and Chelsea	Sheffield
Birmingham	Kent*	Shropshire*
Bolton	Kingston-upon-Hull (City)	Solihull
Bournemouth	Kingston-upon-Thames	Somerset
Bradford	Kirklees	South Gloucestershire
Brent	Knowsley	South Tyneside
Bromley	Lambeth	Southampton
Buckinghamshire	Lancashire*	Southend-on-Sea
Bury	Leeds	Southwark
Calderdale	Leicester (City)	St. Helens
Cambridgeshire*	Leicestershire	Staffordshire
Camden	Lewisham	Stockport
Cornwall	Lincolnshire	Stockton-on-Tees
Coventry	Liverpool	Stoke-on-Trent
Croydon	Luton	Suffolk
Cumbria	Manchester	Sunderland
Darlington	Merton	Surrey
Derby (City)	Middlesbrough	Sutton
Derbyshire	Newcastle upon Tyne	Swindon
Doncaster	Newham	Tameside
Dorset	Norfolk	Torbay
Dudley	North East Lincolnshire	Tower Hamlets
Durham	North Lincolnshire	Wakefield
Ealing	North Somerset	Walsall
East Riding of Yorkshire	North Tyneside	Waltham Forest
East Sussex	North Yorkshire	Wandsworth
Gateshead	Northumberland	Warwickshire
Greenwich	Nottinghamshire*	West Berkshire
Hackney	Oldham	West Sussex
Halton	Oxfordshire	Westminster (City)
Hammersmith and Fulham	Plymouth (City)	Wigan
Hampshire	Poole	Wiltshire
Haringey	Portsmouth	Wirral
Harrow	Redcar and Cleveland	Wokingham
Havering	Richmond-upon-Thames	York (City)

Note: * indicates the five boundary changing LEAs during the sample period; all others have unchanged boundaries throughout.

Table A.2: Local Education Authorities: in additional samples

Sample 2

Local Education Authority	**5-year unchanging**	**boundary changer post-1998**	**New LEA in 1998**	**LEA 1998 childcare data mapped from**
Cheshire	**no**	**yes**		
Devon	**no**	**yes**		
Essex	**no**	**yes**		
Blackburn with Darwen	**no**		**yes**	Lancashire
Blackpool	**no**		**yes**	Lancashire
Medway	**no**		**yes**	Kent
Nottingham (City)	**no**		**yes**	Nottinghamshire
Peterborough (City)	**no**		**yes**	Cambridgeshire
Telford and Wrekin	**no**		**yes**	Shropshire
Thurrock	**no**		**yes**	Essex

'All Observations' Sample 3

Local Education Authority	**5-year unchanging**	**boundary changer post-1998**	**New LEA in 1998**	**LEA 1998 childcare data mapped from**
Bracknell Forest	**yes**	**no**	**yes**	
Brighton and Hove	**yes**	**no**	**no**	
Bristol (City)	**yes**	**no**	**no**	
Enfield	**yes**	**no**	**no**	
Gloucestershire	**yes**	**no**	**no**	
Hartlepool	**yes**	**no**	**no**	
Herefordshire	**yes**	**no**	**yes**	
Milton Keynes	**yes**	**no**	**no**	
Northamptonshire	**yes**	**no**	**no**	
Reading	**yes**	**no**	**yes**	
Redbridge	**yes**	**no**	**no**	
Rotherham	**yes**	**no**	**no**	
Slough	**yes**	**no**	**yes**	
Trafford	**yes**	**no**	**no**	
Warrington	**yes**	**no**	**yes**	Cheshire
Windsor and Maidenhead	**yes**	**no**	**yes**	
Wolverhampton	**yes**	**no**	**no**	
Worcestershire	**yes**	**no**	**yes**	

Table A.3: The poorer LEAs first to receive the policy

LEA	sample 1	sample 2	LEA	sample 1	sample 2
Barking and Dagenham	**yes**	**yes**	Manchester	**yes**	**yes**
Barnsley	**yes**	**yes**	Middlesbrough	**yes**	**yes**
Birmingham	**yes**	**yes**	Newcastle upon Tyne	**yes**	**yes**
Blackburn with Darwen	**no**	**yes**	Newham	**yes**	**yes**
Blackpool	**no**	**yes**	North East Lincolnshire	**yes**	**yes**
Bolton	**yes**	**yes**	North Tyneside	**yes**	**yes**
Bradford	**yes**	**yes**	Northamptonshire	**no**	**no**
Brent	**yes**	**yes**	Nottingham (City)	**no**	**yes**
Brighton and Hove	**no**	**no**	Oldham	**yes**	**yes**
Camden	**yes**	**yes**	Plymouth (City)	**yes**	**yes**
Cornwall	**yes**	**yes**	Rochdale	**yes**	**yes**
Coventry	**yes**	**yes**	Rotherham	**no**	**no**
Doncaster	**yes**	**yes**	Salford	**yes**	**yes**
Durham	**yes**	**yes**	Sandwell	**yes**	**yes**
Ealing	**yes**	**yes**	Sefton	**yes**	**yes**
Gateshead	**yes**	**yes**	Sheffield	**yes**	**yes**
Greenwich	**yes**	**yes**	South Tyneside	**yes**	**yes**
Hackney	**yes**	**yes**	Southwark	**yes**	**yes**
Halton	**yes**	**yes**	St Helens	**yes**	**yes**
Hammersmith and Fulham	**yes**	**yes**	Stockton on Tees	**yes**	**yes**
Haringey	**yes**	**yes**	Stoke on Trent	**yes**	**yes**
Hartlepool	**no**	**no**	Sunderland	**yes**	**yes**
Hounslow	**yes**	**yes**	Tameside	**yes**	**yes**
Isle of Wight	**yes**	**yes**	Telford and Wrekin	**no**	**yes**
Islington	**yes**	**yes**	Tower Hamlets	**yes**	**yes**
Kingston upon Hull (City)	**yes**	**yes**	Wakefield	**yes**	**yes**
Kirklees	**yes**	**yes**	Walsall	**yes**	**yes**
Knowsley	**yes**	**yes**	Waltham Forest	**yes**	**yes**
Lambeth	**yes**	**yes**	Wandsworth	**yes**	**yes**
Leeds	**yes**	**yes**	Westminster (City)	**yes**	**yes**
Leicester (City)	**yes**	**yes**	Wirral	**yes**	**yes**
Lewisham	**yes**	**yes**	Wolverhampton	**no**	**no**
Liverpool	**yes**	**yes**			

A.2 Results Distributions

Figure A.1 (below) shows for each subject and each level kernel density plots of the distribution of results for the main sample of LEAs over the entire time period of the data, with a normal density curve overlaid in each case. Looking at the top row, we can see that for L3 or higher, the distribution of LEA results is close to normal, with the reading and maths distributions very similar. Writing L3 or higher results are almost perfectly normally distributed with a lower mean than the other subjects at this level, but a tighter spread of results around the mean. For L2B or higher results, the lower row shows that again results are distributed for each subject close to a normal distribution but in each case there is a left skewing of the distribution.

Figures A.2 to A.7 show kernel density plots of the overall distributions (both groups of LEAs together) for each subject and each level, with each year plotted separately. Looking at Figure

A.2 we can see that for reading L3 or higher, the shape of the distribution is approximately the same in each year, but that there is movement of the distribution over time – 2003 and 2004 are very similar but there is a definite shift above (2002) and below (2006) in the other two years. Similarly, Figure A.3 shows that the shape of the distribution of reading L2B or higher results remains largely the same over time, though there are definite shifts in certain years, but less clearly than in the L3 or higher results.

Looking at Figures A.4 and A.5, we see that for writing the story is very similar, L3 or higher results having a common shape each year but with clear shifts of the distribution in different years. L2B or higher results for writing has largely the same shaped distribution each year, though with more variation than is the case for reading at this level. Again, though there is movement in the position of the distribution in the different years, it is not as marked as in the L3 or higher results.

Again for Maths, figures A.6 and A.7 show that while the shape of the distribution of results looks very similar in each year, there are very definite shifts of the distribution over time. This is particularly the case for maths at L3 or higher, where there is a large leftward shift in the distribution in 2006.

Figure A.1: Kernel Density Plots of the Distributions of Dependent Variables with Normal Density Overlaid

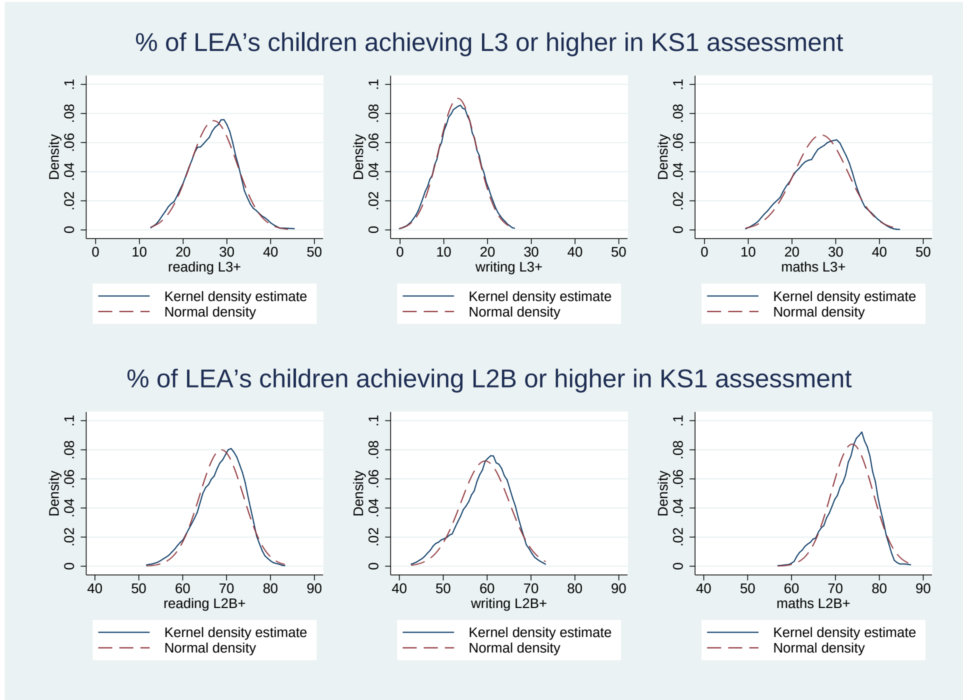


Figure A.2: Kernel Density Plots of the Distribution of Reading L3+, Results by year

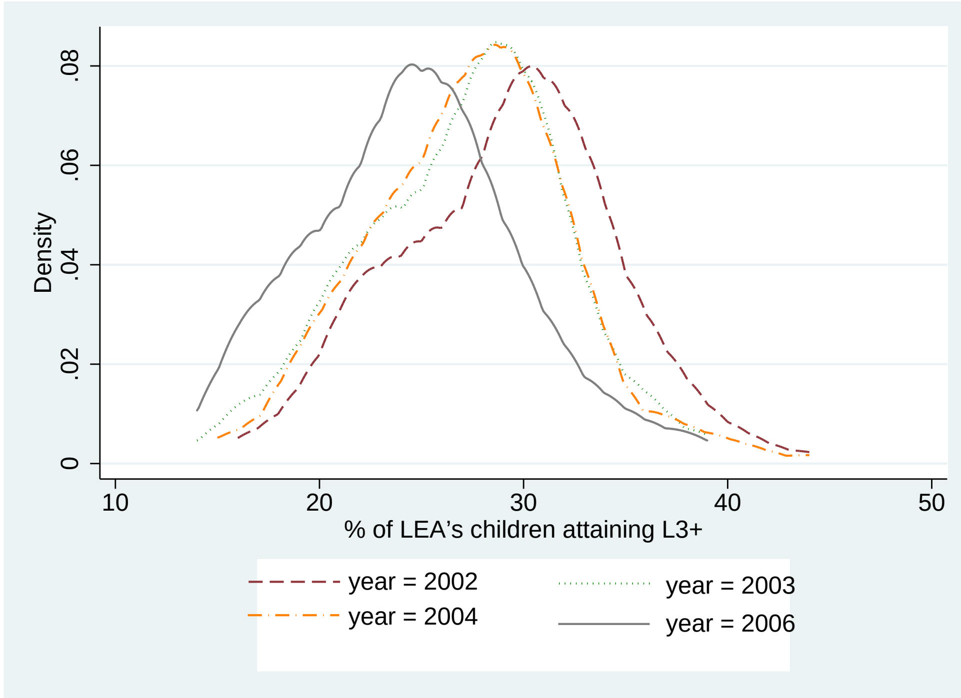


Figure A.3: Kernel Density Plots of the Distribution of Reading L2B+, Results by year

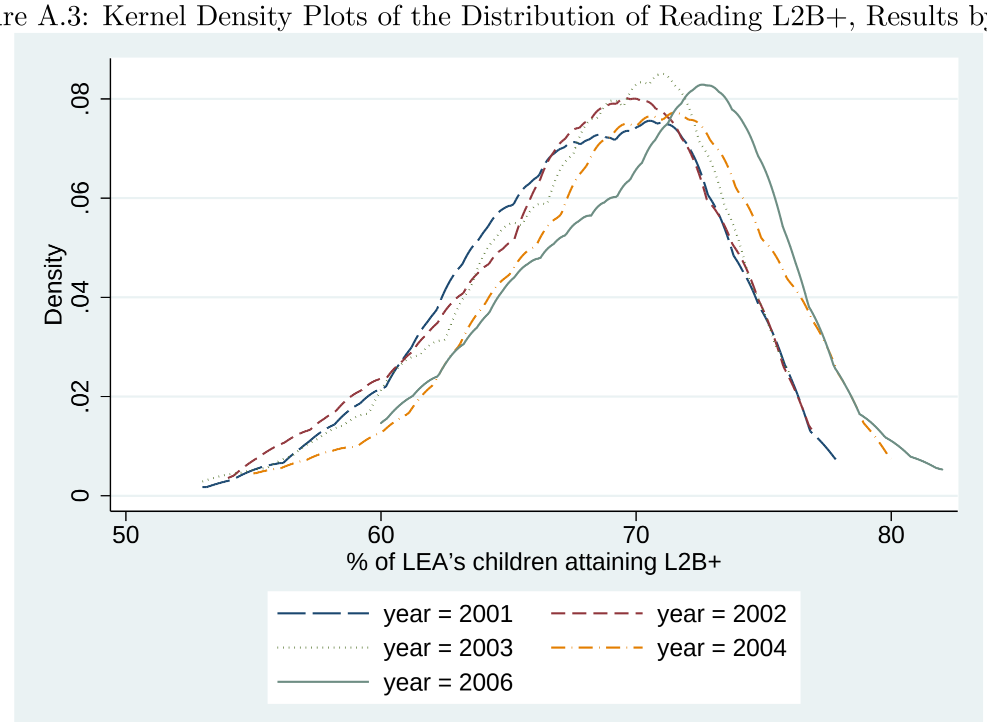


Figure A.4: Kernel Density Plots of the Distribution of Writing L3+, Results by year

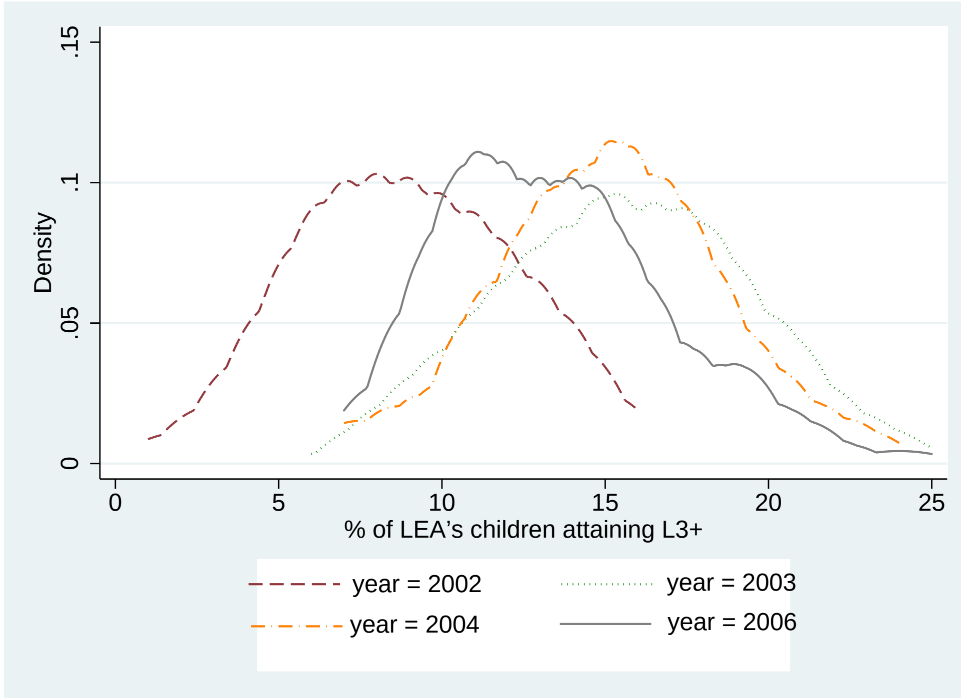


Figure A.5: Kernel Density Plots of the Distribution of Writing L2B+, Results by year

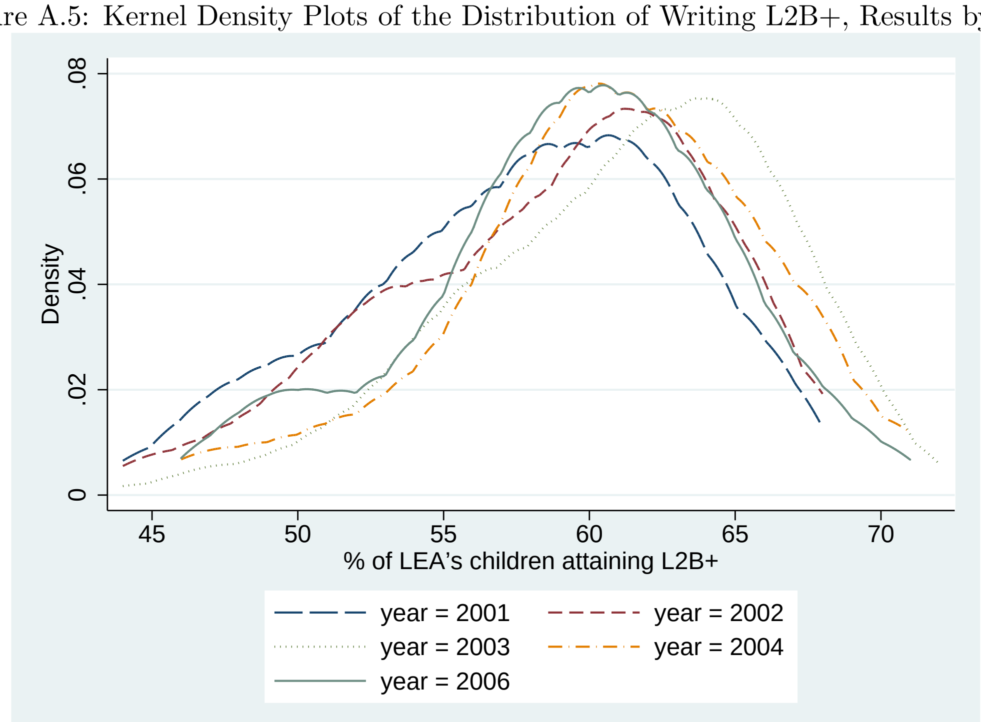


Figure A.6: Kernel Density Plots of the Distribution of Maths L3+, Results by year

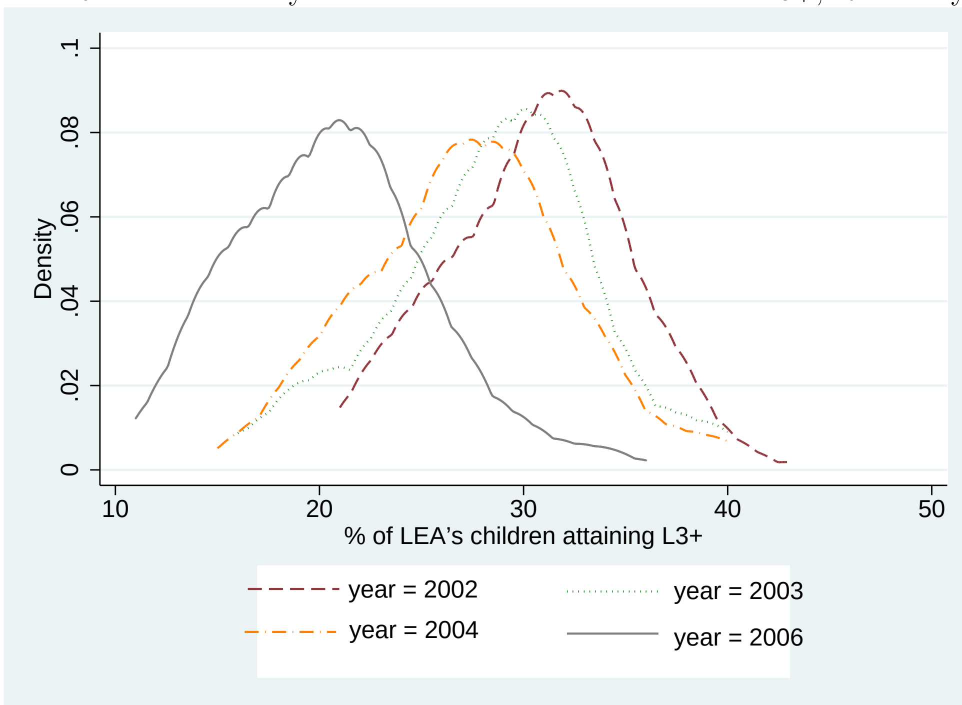
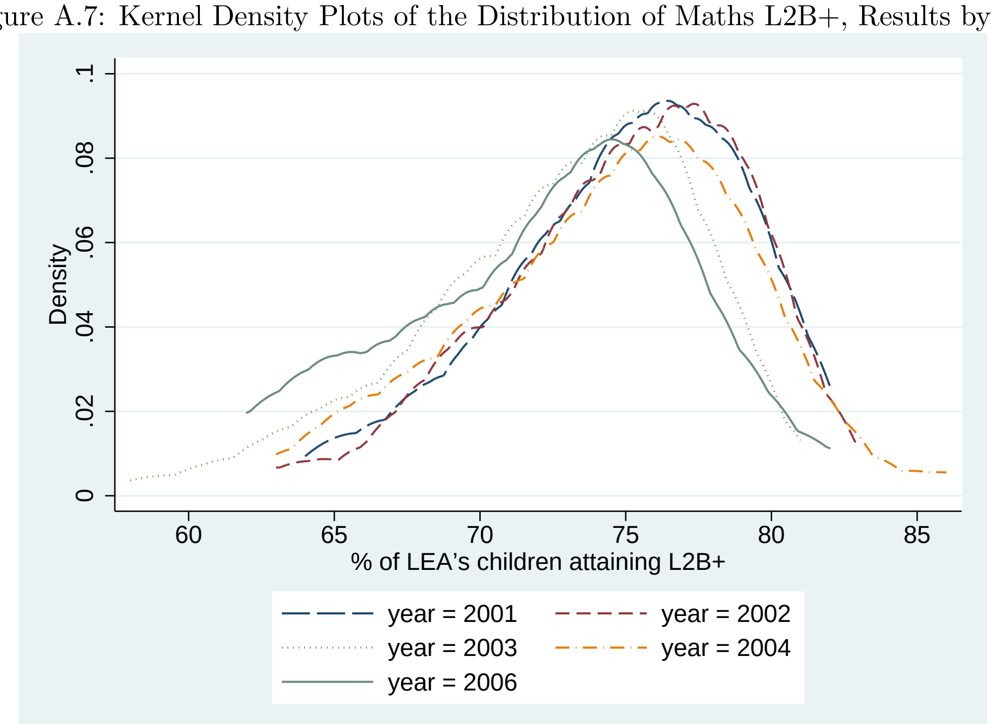


Figure A.7: Kernel Density Plots of the Distribution of Maths L2B+, Results by year



A.3 Robustness: Policy Evaluation Regressions

Tables 2.11 – 2.13 in the main body of the paper report the results from the simple panel regression model in which results in each subject (reading, writing, maths) at each level (2B or higher and 3 or higher) are regressed on a dummy for the free early education places for 3-year olds policy being in operation and a set of year dummies. The results presented in the main body of the paper are for both the model in which there is a single policy dummy and that in which there are separate policy dummies for the poorer and the better off LEAs, in each case for the main estimation sample (sample 1). The Tables below below present the same sample 1 results but also include alongside the results of the same regressions for the larger samples 2 and 3 as a robustness check of the sample 1 results.

The second sample differs from the main estimation sample in that it includes an additional 10 LEAs who are excluded from sample 1 as they have a boundary change mid-1998 or are new mid-1998 and therefore have either slightly different areas of reference for their 1998 observation or only have a 1998 observation through data apportioned by the DfES from their "parent" LEAs. In order to avoid the issues around these observations, I exclude the 1998 observation for these 10 LEAs and as this necessarily further unbalances the panel, I exclude these 10 LEAs from the main estimation sample.

The final sample (sample 3) is the "all observations" sample, in which any observations from an LEA that has all the necessary variables for the regression is included, though continuing to exclude the 1998 observations of the 10 LEAs introduced in the second sample. This means that information from 148 of the 150 LEAs is used.

The second set of tables (Tables A.10 to A.15) then go on to do report the same results – each subject at each level for each sample – but with the data from results year 2006 excluded, to confirm that the results still hold when we exclude this data. I run this robustness check because for the years 2001 to 2004, the published results refer to the children's attainments in a standard national task/test, thus this is a consistent dependent variable. From 2005 onwards however, the assessment altered slightly to be a teacher assessed level for the child – based on their performance in the standard national task/test but also taking into account the teacher's own knowledge of the child. Clearly this is something that could potentially affect results, and could affect things differentially across LEAs depending on the teachers' attitudes within each LEA. In light of the potential problems caused by this alteration in assessment method, I robustness check the results by running the regressions both with and without 2006 results data included.

Table A.4: The Effect of Free Early Education Place Policy on **Reading L2B+**

Dependent Variable: Percentage of children in the LEA's maintained school achieving Level 2B or higher in Key Stage 1 assessment, year (t+3).

Panel Regression Models: 5 year panel

	Model #1			**Model #2**		
Independent Variable	**sample 1**	**sample 2**	**sample 3**	**sample 1**	**sample 2**	**sample 3**
policy (t)	-0.007	-0.117	-0.108			
	0.276	0.270	0.252			
policy 'poorer' LEAs (t)				-0.156	-0.223	-0.286
				0.320	0.306	0.287
policy 'better-off' LEAs (t)				0.138	-0.019	0.055
				0.336	0.335	0.315
year=2002	-0.128	-0.125	-0.016	-0.129	-0.125	-0.011
	0.162	0.164	0.157	0.162	0.164	0.156
year=2003	0.119	0.171	0.238	0.189	0.220	0.321
	0.239	0.239	0.224	0.228	0.226	0.216
year=2004	**1.791*** **	**1.895*** **	**1.934*** **	**1.783*** **	**1.892*** **	**1.928*** **
	0.317	0.310	0.299	0.319	0.312	0.301
year=2006	**2.471*** **	**2.585*** **	**2.672*** **	**2.463*** **	**2.582*** **	**2.664*** **
	0.354	0.344	0.334	0.355	0.345	0.335
constant	**67.969*** **	**67.805*** **	**67.858*** **	**67.969*** **	**67.805*** **	**67.854*** **
	0.140	0.142	0.136	0.140	0.141	0.135
R^2 within	0.397	0.390	0.395	0.398	0.390	0.397***
R^2 between	0.000	0.000	0.003	0.109	0.044	0.110***
R^2 overall	0.045	0.045	0.05	0.065	0.057	0.071***
ρ	0.910	0.908	0.905	0.908	0.907	0.903***
#obs	575	614	692	575	614	692***
#groups	120	130	148	120	130	148***
LEA level fixed effects included	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors, lower figure

ρ is the fraction of the variance due to the fixed effects

* p<0.10, ** p<0.05, *** p<0.01

Table A.5: The Effect of Free Early Education Place Policy on **Writing L2B+**

Dependent Variable: Percentage of children in the LEA's maintained school achieving Level 2B or higher in Key Stage 1 assessment, year (t+3).

Panel Regression Models: 5 year panel

	Model #1			**Model #2**		
Independent Variable	**sample 1**	**sample 2**	**sample 3**	**sample 1**	**sample 2**	**sample 3**
policy (t)	0.255	0.208	0.071			
	0.362	0.352	0.335			
policy 'poorer' LEAs (t)				0.422	0.431	0.271
				0.447	0.431	0.417
policy 'better-off' LEAs (t)				0.093	0.002	-0.112
				0.413	0.405	0.382
year=2002	**0.885*** **	**0.887*** **	**0.944*** **	**0.885*** **	**0.886*** **	**0.939*** **
	0.212	0.210	0.198	0.212	0.211	0.198
year=2003	**3.221*** **	**3.258*** **	**3.386*** **	**3.143*** **	**3.154*** **	**3.292*** **
	0.310	0.308	0.293	0.315	0.310	0.294
year=2004	**2.804*** **	**2.858*** **	**3.054*** **	**2.813*** **	**2.865*** **	**3.061*** **
	0.407	0.396	0.382	0.406	0.396	0.382
year=2006	**1.454*** **	**1.477*** **	**1.807*** **	**1.463*** **	**1.485*** **	**1.816*** **
	0.459	0.443	0.426	0.460	0.444	0.427
constant	**57.752*** **	**57.544*** **	**57.558*** **	**57.752*** **	**57.544*** **	**57.562*** **
	0.166	0.169	0.163	0.166	0.169	0.162
R^2 within	0.355	0.348	0.350	0.356	0.350	0.351
R^2 between	0.001	0.011	0.000	0.069	0.116	0.056
R^2 overall	0.047	0.041	0.048	0.032	0.025	0.032
ρ	0.880	0.880	0.876	0.882	0.883	0.879
#obs	575	614	692	575	614	692
#groups	120	130	148	120	130	148
LEA level fixed effects included	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors, lower figure

ρ is the fraction of the variance due to the fixed effects

* p<0.10, ** p<0.05, *** p<0.01

Table A.6: The Effect of Free Early Education Place Policy on **Maths L2B+**

Dependent Variable: Percentage of children in the LEA's maintained school achieving Level 2B or higher in Key Stage 1 assessment, year (t+3).

Panel Regression Models: 5 year panel

	Model #1			**Model #2**		
Independent Variable	**sample 1**	**sample 2**	**sample 3**	**sample 1**	**sample 2**	**sample 3**
policy (t)	-0.250	-0.384	-0.311			
	0.285	0.285	0.271			
policy 'poorer' LEAs (t)				**-0.722****	**-0.796****	**-0.842*****
				0.331	0.323	0.304
policy 'better-off' LEAs (t)				0.205	-0.004	0.176
				0.330	0.333	0.319
year=2002	-0.110	-0.092	-0.115	-0.111	-0.091	-0.102
	0.159	0.159	0.148	0.159	0.159	0.147
year=2003	**-2.265*****	**-2.221*****	**-2.248*****	**-2.045*****	**-2.028*****	**-1.999*****
	0.213	0.212	0.199	0.224	0.222	0.205
year=2004	-0.149	-0.003	0.038	-0.173	-0.016	0.019
	0.350	0.352	0.336	0.346	0.349	0.333
year=2006	**-2.605*****	**-2.481*****	**-2.472*****	**-2.630*****	**-2.496*****	**-2.498*****
	0.372	0.368	0.353	0.365	0.363	0.346
constant	**74.984*****	**74.886*****	**74.993*****	**74.985*****	**74.884*****	**74.981*****
	0.131	0.132	0.125	0.128	0.130	0.123
R^2 within	0.438	0.434	0.432	0.451	0.443	0.447
R^2 between	0.002	0.012	0.006	0.284	0.250	0.267
R^2 overall	0.068	0.075	0.072	0.138	0.132	0.144
ρ	0.889	0.883	0.882	0.884	0.879	0.877
#obs	575	614	692	575	614	692
#groups	120	130	148	120	130	148
LEA level fixed effects included	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors, lower figure

ρ is the fraction of the variance due to the fixed effects

* p<0.10, ** p<0.05, *** p<0.01

Table A.7: The Effect of Free Early Education Place Policy on **Reading L3+**

Dependent Variable: Percentage of children in the LEA's maintained school achieving Level 3 or higher in Key Stage 1 assessment, year (*t+3*).

Panel Regression Models: 4 year panel

	Model #1			**Model #2**		
Independent Variable	**sample 1**	**sample 2**	**sample 3**	**sample 1**	**sample 2**	**sample 3**
policy (t)	0.184	0.133	0.179			
	0.235	0.223	0.204			
policy 'poorer' LEAs (t)				**0.425†**	**0.482***	0.333
				0.266	0.250	0.240
policy 'better-off' LEAs (t)				0.062	-0.043	0.100
				0.301	0.287	0.269
year=2003	**-2.208*** **	**-2.190*** **	**-2.114*** **	**-2.322*** **	**-2.353*** **	**-2.182*** **
	0.174	0.166	0.154	0.182	0.171	0.168
year=2004	**-1.784*** **	**-1.710*** **	**-1.541*** **	**-1.833*** **	**-1.778*** **	**-1.566*** **
	0.287	0.275	0.258	0.277	0.264	0.252
year=2006	**-4.587*** **	**-4.489*** **	**-4.350*** **	**-4.635*** **	**-4.555*** **	**-4.375*** **
	0.317	0.307	0.289	0.312	0.299	0.285
constant	**29.046*** **	**28.856*** **	**28.895*** **	**29.046*** **	**28.857*** **	**28.896*** **
	0.112	0.107	0.107	0.112	0.106	0.107
R^2 within	0.610	0.604	0.585	0.611	0.607	0.585
R^2 between	0.013	0.01	0.022	0.233	0.285	0.151
R^2 overall	0.083	0.083	0.08	0.058	0.049	0.064
ρ	0.917	0.916	0.911	0.919	0.92	0.913
#obs	464	503	570	464	503	570
#groups	120	130	148	120	130	148
LEA level fixed effects included	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors, lower figure — ρ is the fraction of the variance due to the fixed effects

* p<0.10, ** p<0.05, *** p<0.01 — † p=0.116

Table A.8: The Effect of Free Early Education Place Policy on **Writing L3+**

Dependent Variable: Percentage of children in the LEA's maintained school achieving Level 3 or higher in Key Stage 1 assessment, year (*t+3*).

Panel Regression Models: 4 year panel

	Model #1			**Model #2**		
Independent Variable	**sample 1**	**sample 2**	**sample 3**	**sample 1**	**sample 2**	**sample 3**
policy (t)	-0.057	-0.109	-0.169			
	0.325	0.311	0.290			
policy 'poorer' LEAs (t)				**-0.728†**	**-0.766***	**-0.819****
				0.448	0.423	0.396
policy 'better-off' LEAs (t)				0.282	0.224	0.163
				0.342	0.333	0.311
year=2003	**6.625*****	**6.641*****	**6.743*****	**6.941*****	**6.947*****	**7.033*****
	0.292	0.279	0.254	0.335	0.312	0.281
year=2004	**6.268*****	**6.318*****	**6.466*****	**6.403*****	**6.445*****	**6.573*****
	0.396	0.380	0.353	0.406	0.386	0.356
year=2006	**4.612*****	**4.676*****	**4.810*****	**4.746*****	**4.801*****	**4.913*****
	0.413	0.390	0.354	0.423	0.396	0.358
constant	**8.911*****	**8.761*****	**8.789*****	**8.909*****	**8.760*****	**8.788*****
	0.155	0.146	0.138	0.152	0.144	0.135
R^2 within	0.76	0.759	0.766	0.764	0.764	0.77
R^2 between	0.007	0.005	0.015	0.187	0.156	0.17
R^2 overall	0.353	0.349	0.364	0.41	0.399	0.413
ρ	0.780	0.783	0.778	0.765	0.770	0.765
#obs	464	503	570	464	503	570
#groups	120	130	148	120	130	148
LEA level fixed effects included	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors, lower figure — ρ is the fraction of the variance due to the fixed effects

* p<0.10, ** p<0.05, *** p<0.01 † p=0.107

Table A.9: The Effect of Free Early Education Place Policy on **Maths L3+**

Dependent Variable: Percentage of children in the LEA's maintained school achieving Level 3 or higher in Key Stage 1 assessment, year (*t+3*).

Panel Regression Models: 4 year panel

	Model #1			**Model #2**		
Independent Variable	**sample 1**	**sample 2**	**sample 3**	**sample 1**	**sample 2**	**sample 3**
policy (t)	-0.469	-0.479	-0.377			
	0.346	0.331	0.321			
policy 'poorer' LEAs (t)				**-0.825****	**-0.778****	**-0.754****
				0.407	0.387	0.357
policy 'better-off' LEAs (t)				-0.289	-0.327	-0.184
				0.389	0.371	0.365
year=2003	**-2.002*****	**-2.080*****	**-2.076*****	**-1.834*****	**-1.941*****	**-1.908*****
	0.251	0.241	0.228	0.290	0.277	0.252
year=2004	**-2.803*****	**-2.783*****	**-2.669*****	**-2.731*****	**-2.725*****	**-2.606*****
	0.422	0.405	0.382	0.425	0.408	0.381
year=2006	**-9.516*****	**-9.476*****	**-9.446*****	**-9.445*****	**-9.419*****	**-9.386*****
	0.447	0.424	0.397	0.452	0.428	0.398
constant	**30.592*****	**30.442*****	**30.517*****	**30.591*****	**30.441*****	**30.516*****
	0.138	0.129	0.120	0.137	0.128	0.119
R^2 within	0.841	0.844	0.842	0.842	0.845	0.843
R^2 between	0.016	0.014	0.003	0.185	0.141	0.115
R^2 overall	0.384	0.386	0.387	0.412	0.409	0.414
ρ	0.849	0.851	0.846	0.842	0.845	0.840
#obs	464	503	570	464	503	570
#groups	120	130	148	120	130	148
LEA level fixed effects included	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors, lower figure

* p<0.10, ** p<0.05, *** p<0.01

ρ is the fraction of the variance due to the fixed effects

Table A.10: The Effect of Free Early Education Place Policy on **Reading L2B+**; 2006 excluded

Data from 2006 excluded. **Dependent Variable:** Percentage of children in the LEA's maintained school achieving Level 2B or higher in Key Stage 1 assessment, year (*t+3*).

Panel Regression Models: 4 year panel

	Model #1			**Model #2**		
Independent Variable	**sample 1**	**sample 2**	**sample 3**	**sample 1**	**sample 2**	**sample 3**
policy (t)	-0.130	-0.181	-0.232			
	0.288	0.277	0.260			
policy 'poorer' LEAs (t)				-0.168	-0.210	-0.271
				0.321	0.306	0.287
policy 'better-off' LEAs (t)				-0.055	-0.126	-0.158
				0.365	0.355	0.331
year=2002	-0.101	-0.099	0.007	-0.101	-0.099	0.008
	0.161	0.161	0.154	0.161	0.161	0.153
year=2003	0.191	0.214	0.305	0.208	0.227	0.323
	0.236	0.232	0.220	0.230	0.224	0.215
year=2004	**1.939*** **	**1.989*** **	**2.080*** **	**1.916*** **	**1.973*** **	**2.057*** **
	0.316	0.306	0.296	0.329	0.319	0.309
constant	**67.945*** **	**67.786*** **	**67.837*** **	**67.945*** **	**67.786*** **	**67.836*** **
	0.126	0.128	0.122	0.126	0.129	0.121
R^2 within	0.309	0.313	0.313	0.309	0.313	0.313
R^2 between	0.000	0.001	0.005	0.010	0.001	0.020
R^2 overall	0.026	0.024	0.029	0.031	0.028	0.034
ρ	0.925	0.926	0.925	0.924	0.926	0.924
#obs	456	485	545	456	485	545
#groups	120	130	148	120	130	148
LEA level fixed effects included	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors, lower figure

* p<0.10, ** p<0.05, *** p<0.01

ρ is the fraction of the variance due to the fixed effects

Table A.11: The Effect of Free Early Education Place Policy on **Writing L2B+**; 2006 excluded

Data from 2006 excluded. **Dependent Variable:** Percentage of children in the LEA's maintained school achieving Level 2B or higher in Key Stage 1 assessment, year (*t+3*).

Panel Regression Models: 4 year panel

	Model #1			**Model #2**		
Independent Variable	**sample 1**	**sample 2**	**sample 3**	**sample 1**	**sample 2**	**sample 3**
policy (t)	0.131	0.126	-0.008			
	0.385	0.369	0.354			
policy 'poorer' LEAs (t)				0.403	0.430	0.277
				0.448	0.430	0.416
policy 'better-off' LEAs (t)				-0.407	-0.453	-0.545
				0.436	0.422	0.394
year=2002	**0.901*** **	**0.893*** **	**0.937*** **	**0.903*** **	**0.894*** **	**0.930*** **
	0.212	0.210	0.197	0.212	0.210	0.197
year=2003	**3.280*** **	**3.288*** **	**3.402*** **	**3.154*** **	**3.147*** **	**3.269*** **
	0.313	0.305	0.288	0.316	0.307	0.290
year=2004	**2.935*** **	**2.944*** **	**3.123*** **	**3.098*** **	**3.113*** **	**3.286*** **
	0.426	0.410	0.394	0.421	0.406	0.389
constant	**57.739*** **	**57.544*** **	**57.562*** **	**57.738*** **	**57.544*** **	**57.567*** **
	0.149	0.152	0.146	0.149	0.152	0.145
R^2 within	0.453	0.451	0.456	0.459	0.459	0.463
R^2 between	0.001	0.003	0.001	0.057	0.085	0.041
R^2 overall	0.061	0.053	0.061	0.038	0.031	0.039
ρ	0.894	0.897	0.894	0.898	0.901	0.898
#obs	456	485	545	456	485	545
#groups	120	130	148	120	130	148
LEA level fixed effects included	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors, lower figure

ρ is the fraction of the variance due to the fixed effects

* p<0.10, ** p<0.05, *** p<0.01

Table A.12: The Effect of Free Early Education Place Policy on **Maths L2B+**; 2006 excluded

Data from 2006 excluded. **Dependent Variable:** Percentage of children in the LEA's maintained school achieving Level 2B or higher in Key Stage 1 assessment, year (*t+3*).

Panel Regression Models: 4 year panel

	Model #1			Model #2		
Independent Variable	**sample 1**	**sample 2**	**sample 3**	**sample 1**	**sample 2**	**sample 3**
policy (t)	**-0.599****	**-0.694****	**-0.654****			
	0.294	0.290	0.276			
policy 'poorer' LEAs (t)				**-0.752****	**-0.805****	**-0.839*****
				0.329	0.321	0.302
policy 'better-off' LEAs (t)				-0.295	-0.484	-0.304
				0.354	0.350	0.336
year=2002	-0.095	-0.080	-0.102	-0.096	-0.080	-0.097
	0.159	0.157	0.146	0.159	0.157	0.146
year=2003	**-2.071*****	**-2.050*****	**-2.072*****	**-2.000*****	**-1.999*****	**-1.985*****
	0.209	0.204	0.190	0.222	0.217	0.200
year=2004	0.217	0.320	0.390	0.125	0.259	0.284
	0.361	0.359	0.341	0.352	0.354	0.337
constant	**74.961*****	**74.867*****	**74.970*****	**74.961*****	**74.867*****	**74.966*****
	0.119	0.121	0.114	0.119	0.121	0.114
R^2 within	0.408	0.421	0.427	0.412	0.423	0.432
R^2 between	0.053	0.073	0.062	0.160	0.144	0.166
R^2 overall	0.062	0.069	0.068	0.087	0.086	0.095
ρ	0.910	0.910	0.908	0.909	0.908	0.907
#obs	456	485	545	456	485	545
#groups	120	130	148	120	130	148
LEA level fixed effects included	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors, lower figure

* p<0.10, ** p<0.05, *** p<0.01

ρ is the fraction of the variance due to the fixed effects

Table A.13: The Effect of Free Early Education Place Policy on **Reading L3+**; 2006 excluded

Data from 2006 excluded. **Dependent Variable:** Percentage of children in the LEA's maintained school achieving Level 3 or higher in Key Stage 1 assessment, year (*t+3*).

Panel Regression Models: 3 year panel

	Model #1			Model #2		
Independent Variable	**sample 1**	**sample 2**	**sample 3**	**sample 1**	**sample 2**	**sample 3**
policy (t)	0.191	0.204	0.187			
	0.228	0.215	0.199			
policy 'poorer' LEAs (t)				0.373	**0.436***	0.296
				0.265	0.249	0.240
policy 'better-off' LEAs (t)				0.000	-0.040	0.070
				0.311	0.293	0.277
year=2003	**-2.181*****	**-2.195*****	**-2.094*****	**-2.267*****	**-2.303*****	**-2.142*****
	0.172	0.162	0.154	0.181	0.170	0.167
year=2004	**-1.772*****	**-1.762*****	**-1.541*****	**-1.756*****	**-1.740*****	**-1.526*****
	0.278	0.262	0.252	0.280	0.264	0.253
constant	**29.028*****	**28.834*****	**28.871*****	**29.028*****	**28.834*****	**28.872*****
	0.089	0.084	0.083	0.089	0.084	0.083
R^2 within	0.471	0.482	0.437	0.474	0.487	0.438
R^2 between	0.016	0.015	0.013	0.181	0.210	0.104
R^2 overall	0.022	0.023	0.022	0.007	0.005	0.011
ρ	0.948	0.950	0.944	0.949	0.951	0.945
#obs	345	374	423	345	374	423
#groups	120	130	148	120	130	148
LEA level fixed effects included	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors, lower figure

ρ is the fraction of the variance due to the fixed effects

* p<0.10, ** p<0.05, *** p<0.01

Table A.14: The Effect of Free Early Education Place Policy on **Writing L3+**; 2006 excluded

Data from 2006 excluded. **Dependent Variable:** Percentage of children in the LEA's maintained school achieving Level 3 or higher in Key Stage 1 assessment, year (*t+3*).

Panel Regression Models: 3 year panel

	Model #1			Model #2		
Independent Variable	**sample 1**	**sample 2**	**sample 3**	**sample 1**	**sample 2**	**sample 3**
policy (t)	-0.433	-0.450	-0.465			
	0.338	0.319	0.297			
policy 'poorer' LEAs (t)				**-0.776***	**-0.809***	**-0.865****
				0.449	0.423	0.397
policy 'better-off' LEAs (t)				-0.076	-0.073	-0.041
				0.341	0.332	0.306
year=2003	**6.812*****	**6.809*****	**6.888*****	**6.974*****	**6.976*****	**7.066*****
	0.300	0.283	0.256	0.336	0.313	0.282
year=2004	**6.631*****	**6.646*****	**6.753*****	**6.601*****	**6.612*****	**6.699*****
	0.406	0.385	0.355	0.398	0.380	0.347
constant	**8.908*****	**8.756*****	**8.778*****	**8.908*****	**8.756*****	**8.777*****
	0.135	0.129	0.121	0.134	0.128	0.120
R^2 within	0.853	0.853	0.857	0.855	0.855	0.860
R^2 between	0.035	0.028	0.042	0.103	0.088	0.113
R^2 overall	0.432	0.425	0.441	0.457	0.448	0.467
ρ	0.816	0.819	0.814	0.810	0.814	0.809
#obs	345	374	423	345	374	423
#groups	120	130	148	120	130	148
LEA level fixed effects included	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors, lower figure

ρ is the fraction of the variance due to the fixed effects

* p<0.10, ** p<0.05, *** p<0.01

Table A.15: The Effect of Free Early Education Place Policy on **Maths L3+**; 2006 excluded

Data from 2006 excluded. **Dependent Variable:** Percentage of children in the LEA's maintained school achieving Level 3 or higher in Key Stage 1 assessment, year (*t+3*).

Panel Regression Models: 3 year panel

	Model #1			**Model #2**		
Independent Variable	**sample 1**	**sample 2**	**sample 3**	**sample 1**	**sample 2**	**sample 3**
policy (t)	-0.444	-0.469	-0.359			
	0.374	0.357	0.341			
policy 'poorer' LEAs (t)				**-0.858****	**-0.808****	**-0.778****
				0.410	0.389	0.360
policy 'better-off' LEAs (t)				-0.013	-0.113	0.087
				0.451	0.430	0.421
year=2003	**-1.994*****	**-2.066*****	**-2.068*****	**-1.799*****	**-1.908*****	**-1.881*****
	0.265	0.253	0.237	0.293	0.279	0.254
year=2004	**-2.836*****	**-2.802*****	**-2.706*****	**-2.872*****	**-2.835*****	**-2.764*****
	0.447	0.429	0.402	0.446	0.428	0.402
constant	**30.575*****	**30.422*****	**30.499*****	**30.574*****	**30.422*****	**30.499*****
	0.120	0.112	0.104	0.118	0.111	0.102
R^2 within	0.521	0.531	0.496	0.529	0.537	0.505
R^2 between	0.076	0.086	0.034	0.321	0.276	0.235
R^2 overall	0.094	0.098	0.085	0.154	0.143	0.141
ρ	0.889	0.891	0.884	0.884	0.887	0.88
#obs	345	374	423	345	374	423
#groups	120	130	148	120	130	148
LEA level fixed effects included	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors, lower figure; ρ is the fraction of the variance due to the fixed effects

* p<0.10, ** p<0.05, *** p<0.01

A.4 Robustness: Models #1 & #2

The main estimation sample consists of 120 of the 150 LEAs in England. For analysis of level 2B or higher results there are 575 observations in total, with each LEA contributing between 3 and 5 observations, with a mean of 4.79 observations per LEA. For level 3 analysis there are a total of 464 observations, with each LEA contributing between 3 and 4 observations with a mean of 3.87 observations per LEA. The reason for the slight unbalancing of the panel is that there are some observations (25 in the 5-year panel, 16 in the 4-year panel) that are data mis-reporting or coding errors resulting in large outlying values, which I exclude. The exclusion of some LEAs results in the loss of a number of the group of 65 LEAs who were first given the NEG for 3-year olds. Remaining in the sample of 120 LEAs are 57 of the most deprived 65 LEAs, thus they make up 47% of the estimation sample of LEAs.

The second estimation sample includes an additional 10 LEAs who were either boundary changing LEAs[4] or new LEAs in 1998[5]. The seven LEAs that are new in 1998, have free early education place take-up data – applied via apportionment by the DfES – and population data, however they do not have any childcare availability data for 1998. Rather than making assumptions about these LEAs in 1998 and mapping data to them from their 'parent' LEAs, I exclude the 1998 observation of these seven new LEAs. Similarly, the 1998 observations of the three additional boundary changer LEAs added in this sample are excluded due to differences in the geographical areas that their early education variables and childcare availability variables refer to in 1998. Therefore, though the second sample adds 10 LEAs, for each of them their 1998 observation is excluded. This obviously unbalances the panel somewhat hence the exclusion of these LEAs from the main sample.

As with the main estimation sample, in the second sample, each LEA again contributes between 3 and 5 observations with a mean of 4.72 observations per LEA for the level 2B or higher analysis, and a mean of 3.87 observations per LEA for the level 3 or higher analysis. In this second sample, three of the additional 10 LEAs are from the first 65 group, so they comprise 60 of 130 LEAs in this sample, which is 46% of the sample of LEAs.

There are 2 LEAs (City of London and Isles of Scilly) that are never used because they never

[4] Cheshire, Devon, Essex

[5] Blackburn with Darwen, Blackpool, Medway, Nottingham (City), Peterborough (City), Telford and Wrekin, Thurrock

have sufficient data. The remaining 18 that are not in the main or second samples are excluded because they have missing data in one or more years and thus cannot provide 5 observations to keep the panel largely balanced.

Finally sample 3 is the "all observations" sample, in which any observations from an LEA that has all the necessary variables for the regression is included, though continuing to exclude the 1998 observations of the 10 LEAs introduced in the second sample. This means that information from 148 of the 150 LEAs is used with each contributing between 2 and 5 observations, with an average of 4.59 observations per LEA for the level 2B or higher analysis, and 3.82 observations per LEA for the level 3 or higher analysis. Clearly the panel is unbalanced in this case, and potentially biased by differential attrition, however as a robustness check of the results this sample is included. With 148 LEAs included, all 65 of the group first to receive the NEG for 3-year olds are included, making up 44% of the sample of LEAs.

Table A.16: The Effect of Free Early Education Places on **Reading L2B+**

Dependent Variable: Percentage of children in the LEA's maintained school achieving Level 2B or higher in Key Stage 1 assessment, year (*t+3*).

Panel Regression Models: 5 year panel

	sample 1		**sample 2**		**sample 3**	
Independent Variable	Coeff.	Rob. St. Err.	Coeff.	Rob. St. Err.	Coeff.	Rob. St. Err.
3-year olds early educ take-up, school sector (t)	**0.037****	0.017	**0.046****	0.018	**0.045****	0.018
3-year olds early educ take-up, 'other' sector (t)	0.000	0.008	-0.006	0.007	-0.003	0.007
4-year olds early educ take-up, school sector (t+1)	-0.013	0.046	-0.023	0.045	-0.026	0.043
4-year olds early educ take-up, 'other' sector (t+1)	-0.001	0.023	-0.010	0.023	-0.002	0.023
childcare places available: day nursery (t)	0.015	0.037	0.006	0.040	-0.002	0.037
childcare places available: childminder (t)	0.052	0.049	0.037	0.048	0.040	0.045
childcare places available: playgroup (t)	-0.011	0.017	-0.010	0.017	-0.019	0.015
average KS1 class size (t+3)	**-0.651*****	0.161	**-0.574*****	0.158	**-0.543*****	0.149
year=2002	-0.168	0.163	-0.174	0.166	-0.155	0.166
year=2003	0.230	0.213	0.240	0.218	0.205	0.212
year=2004	**1.969*****	0.267	**2.046*****	0.275	**1.962*****	0.263
year=2006	**2.895*****	0.458	**3.096*****	0.476	**2.974*****	0.454
constant	**83.634*****	5.509	**82.254*****	5.555	**81.991*****	5.307
R^2 within	0.432		0.421		0.420	
R^2 between	0.007		0.037		0.034	
R^2 overall	0.012		0.000		0.000	
ρ	0.919		0.922		0.920	
#obs	575		614		679	
#groups	120		130		148	
LEA level fixed effects included	Yes		Yes		Yes	

ρ is the fraction of the variance due to the fixed effects

* p<0.10, ** p<0.05, *** p<0.01

Table A.17: The Effect of Free Early Education Places on **Reading L2B+**

Dependent Variable: Percentage of children in the LEA's maintained school achieving Level 2B or higher in Key Stage 1 assessment, year (*t+3*).

Panel Regression Models: 5 year panel

	sample 1		sample 2		sample 3	
Independent Variable	Coeff.	Rob. St. Err.	Coeff.	Rob. St. Err.	Coeff.	Rob. St. Err.
3-year olds EE take-up, sch sector 'poorer' LEAs (t)	**0.059****	0.024	**0.072*****	0.025	**0.058****	0.027
3-year olds EE take-up, sch sector 'better-off' LEAs (t)	0.037	0.023	**0.044***	0.024	**0.043***	0.023
3-year olds EE take-up, 'other' sector 'poorer' LEAs (t)	-0.013	0.015	-0.012	0.013	-0.010	0.012
3-year olds EE take-up, 'other' sector 'better-off' LEAs (t)	0.003	0.007	-0.003	0.008	-0.001	0.007
4-year olds EE take-up, sch sector 'poorer' LEAs (t+1)	-0.005	0.058	-0.004	0.053	-0.007	0.051
4-year olds EE take-up, sch sector 'better-off' LEAs (t+1)	-0.021	0.068	-0.050	0.072	-0.047	0.066
4-year olds EE take-up, 'other' sector 'poorer' LEAs (t+1)	0.027	0.038	0.010	0.040	0.009	0.041
4-year olds EE take-up, 'other' sector 'better-off' LEAs (t+1)	0.000	0.028	-0.013	0.029	0.002	0.030
childcare places available: day nursery (t)	0.014	0.036	0.004	0.039	-0.004	0.037
childcare places available: childminder (t)	0.060	0.052	0.044	0.050	0.046	0.047
childcare places available: playgroup (t)	-0.012	0.016	-0.010	0.016	-0.018	0.015
economic inactivity rate, working age males (t)	**-0.057****	0.026	**-0.056****	0.026	**-0.042***	0.024
manufacturing jobs rate (t)	**-0.046***	0.027	-0.030	0.026	-0.025	0.024
average KS1 class size (t+3)	**-0.668*****	0.164	**-0.571*****	0.159	**-0.552*****	0.150
year=2002	-0.222	0.171	-0.224	0.178	-0.199	0.178
year=2003	0.248	0.236	0.208	0.234	0.212	0.226
year=2004	**1.935*****	0.315	**1.987*****	0.306	**1.952*****	0.288
year=2006	**2.843*****	0.524	**3.021*****	0.515	**2.948*****	0.484
constant	**85.024*****	5.254	**83.227*****	5.286	**83.042*****	5.127
R^2 within	0.447		0.433		0.428	
R^2 between	0.189		0.282		0.238	
R^2 overall	0.053		0.161		0.114	
ρ	0.941		0.958		0.948	
#obs	575		614		679	
#groups	120		130		148	
LEA level fixed effects included	Yes		Yes		Yes	

ρ is the fraction of the variance due to the fixed effects

* $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table A.18: The Effect of Free Early Education Places on **Writing L2B+**

Dependent Variable: Percentage of children in the LEA's maintained school achieving Level 2B or higher in Key Stage 1 assessment, year (*t+3*).

Panel Regression Models: 5 year panel

	sample 1		sample 2		sample 3	
Independent Variable	Coeff.	Rob. St. Err.	Coeff.	Rob. St. Err.	Coeff.	Rob. St. Err.
3-year olds EE take-up, sch sector 'poorer' LEAs (t)	**0.070****	0.031	**0.072****	0.03	**0.071*****	0.027
3-year olds EE take-up, sch sector 'better-off' LEAs (t)	-0.012	0.020	-0.003	0.022	-0.013	0.024
3-year olds EE take-up, 'other' sector 'poorer' LEAs (t)	-0.005	0.017	0.002	0.015	0.005	0.015
3-year olds EE take-up, 'other' sector 'better-off' LEAs (t)	0.012	0.011	0.007	0.011	0.007	0.010
4-year olds EE take-up, sch sector 'poorer' LEAs (t+1)	-0.056	0.061	-0.022	0.057	-0.027	0.056
4-year olds EE take-up, sch sector 'better-off' LEAs (t+1)	0.009	0.073	-0.023	0.079	-0.009	0.071
4-year olds EE take-up, 'other' sector 'poorer' LEAs (t+1)	0.029	0.057	0.000	0.052	-0.014	0.055
4-year olds EE take-up, 'other' sector 'better-off' LEAs (t+1)	0.020	0.033	0.012	0.034	0.041	0.034
childcare places available: day nursery (t)	0.008	0.045	0.005	0.047	-0.007	0.041
childcare places available: childminder (t)	-0.060	0.060	-0.060	0.056	-0.052	0.052
childcare places available: playgroup (t)	-0.009	0.019	-0.010	0.020	-0.018	0.018
economic inactivity rate, working age males (t)	-0.047	0.029	-0.042	0.029	-0.028	0.028
manufacturing jobs rate (t)	-0.061	0.039	-0.041	0.037	-0.025	0.035
average KS1 class size (t+3)	**-0.948*****	0.228	**-0.909*****	0.223	**-0.784*****	0.221
year=2002	**0.754*****	0.230	**0.754*****	0.228	**0.708*****	0.222
year=2003	**3.433*****	0.350	**3.393*****	0.336	**3.363*****	0.315
year=2004	**3.084*****	0.426	**3.103*****	0.408	**3.110*****	0.384
year=2006	**2.048*****	0.717	**2.091*****	0.698	**2.156*****	0.654
constant	**84.247*****	6.243	**82.664*****	6.584	**78.821*****	6.624
R^2 within	0.410		0.397		0.380	
R^2 between	0.162		0.216		0.108	
R^2 overall	0.189		0.064		0.010	
ρ	0.869		0.925		0.907	
#obs	575		614		679	
#groups	120		130		148	
LEA level fixed effects included	Yes		Yes		Yes	

ρ is the fraction of the variance due to the fixed effects

* p<0.10, ** p<0.05, *** p<0.01

Table A.19: The Effect of Free Early Education Places on **Maths L2B+**

Dependent Variable: Percentage of children in the LEA's maintained school achieving Level 2B or higher in Key Stage 1 assessment, year (*t+3*).

Panel Regression Models: 5 year panel

	sample 1		sample 2		sample 3	
Independent Variable	Coeff.	Rob. St. Err.	Coeff.	Rob. St. Err.	Coeff.	Rob. St. Err.
3-year olds EE take-up, sch sector 'poorer' LEAs (t)	-0.021	0.037	-0.005	0.042	-0.010	0.039
3-year olds EE take-up, sch sector 'better-off' LEAs (t)	-0.014	0.034	-0.005	0.036	0.010	0.040
3-year olds EE take-up, 'other' sector 'poorer' LEAs (t)	-0.020	0.014	-0.013	0.012	-0.013	0.012
3-year olds EE take-up, 'other' sector 'better-off' LEAs (t)	**0.018****	0.008	0.011	0.009	0.012	0.009
4-year olds EE take-up, sch sector 'poorer' LEAs (t+1)	-0.022	0.054	-0.001	0.051	-0.002	0.050
4-year olds EE take-up, sch sector 'better-off' LEAs (t+1)	0.060	0.049	0.027	0.056	0.041	0.050
4-year olds EE take-up, 'other' sector 'poorer' LEAs (t+1)	0.056	0.040	0.007	0.045	-0.003	0.045
4-year olds EE take-up, 'other' sector 'better-off' LEAs (t+1)	-0.028	0.029	-0.032	0.029	-0.029	0.025
childcare places available: day nursery (t)	-0.033	0.038	-0.039	0.043	-0.025	0.039
childcare places available: childminder (t)	0.067	0.047	0.044	0.047	0.048	0.043
childcare places available: playgroup (t)	-0.022	0.018	-0.015	0.017	-0.012	0.016
economic inactivity rate, working age males (t)	-0.010	0.023	-0.004	0.024	-0.011	0.024
manufacturing jobs rate (t)	**-0.052***	0.029	-0.028	0.031	-0.028	0.028
average KS1 class size (t+3)	**-0.425****	0.164	**-0.311***	0.177	-0.267	0.167
% of non-white children (t+3)	**-0.120*****	0.045	**-0.124*****	0.047	**-0.093****	0.044
year=2002	0.041	0.186	0.042	0.188	-0.028	0.180
year=2003	**-1.766*****	0.236	**-1.854*****	0.256	**-1.984*****	0.246
year=2004	0.200	0.340	0.241	0.348	0.194	0.335
year=2006	**-2.023*****	0.535	**-1.907*****	0.552	**-2.118*****	0.530
constant	**88.156*****	4.680	**84.937*****	5.350	**82.674*****	5.165
R^2 within	0.482		0.458		0.456	
R^2 between	0.627		0.553		0.562	
R^2 overall	0.599		0.541		0.554	
ρ	0.809		0.777		0.767	
#obs	574		613		678	
#groups	120		130		148	
LEA level fixed effects included	Yes		Yes		Yes	

ρ is the fraction of the variance due to the fixed effects

* p<0.10, ** p<0.05, *** p<0.01

Table A.20: The Effect of Free Early Education Places on **Reading L3+**

Dependent Variable: Percentage of children in the LEA's maintained school achieving Level 3 or higher in Key Stage 1 assessment, year (*t+3*).

Panel Regression Models: 4 year panel

	sample 1		**sample 2**		**sample 3**	
Independent Variable	Coeff.	Rob. St. Err.	Coeff.	Rob. St. Err.	Coeff.	Rob. St. Err.
3-year olds EE take-up, sch sector 'poorer' LEAs (t)	0.033	0.043	0.072	0.044	0.052	0.037
3-year olds EE take-up, sch sector 'better-off' LEAs (t)	-0.024	0.037	-0.013	0.038	-0.003	0.036
3-year olds EE take-up, 'other' sector 'poorer' LEAs (t)	0.000	0.012	0.007	0.010	0.006	0.010
3-year olds EE take-up, 'other' sector 'better-off' LEAs (t)	0.003	0.010	-0.002	0.010	0.000	0.009
4-year olds EE take-up, sch sector 'poorer' LEAs (t+1)	-0.055	0.050	-0.058	0.044	-0.060	0.040
4-year olds EE take-up, sch sector 'better-off' LEAs (t+1)	**0.159****	0.069	**0.123***	0.071	0.090	0.072
4-year olds EE take-up, 'other' sector 'poorer' LEAs (t+1)	0.047	0.033	0.018	0.036	0.000	0.037
4-year olds EE take-up, 'other' sector 'better-off' LEAs (t+1)	0.011	0.040	0.007	0.039	0.004	0.033
childcare places available: day nursery (t)	-0.012	0.040	-0.012	0.045	0.002	0.041
childcare places available: childminder (t)	-0.020	0.043	-0.038	0.045	-0.047	0.043
childcare places available: playgroup (t)	-0.015	0.020	-0.002	0.019	-0.005	0.018
economic inactivity rate, working age males (t)	-0.031	0.028	-0.039	0.027	-0.023	0.026
median weekly gross pay, male f-t workers (t)	**0.009***	0.005	**0.011****	0.005	**0.012*****	0.005
average KS1 class size (t+3)	**-0.779*****	0.166	**-0.649*****	0.165	**-0.531*****	0.170
year=2003	**-2.007*****	0.190	**-2.153*****	0.184	**-2.128*****	0.175
year=2004	**-1.557*****	0.274	**-1.685*****	0.265	**-1.578*****	0.254
year=2006	**-4.220*****	0.510	**-4.340*****	0.493	**-4.452*****	0.471
constant	**41.508*****	6.189	**37.643*****	6.100	**35.799*****	5.591
R^2 within	0.655		0.644		0.615	
R^2 between	0.453		0.384		0.331	
R^2 overall	0.462		0.413		0.364	
ρ	0.932		0.900		0.895	
#obs	464		503		566	
#groups	120		130		148	
LEA level fixed effects included	Yes		Yes		Yes	

ρ is the fraction of the variance due to the fixed effects

* p<0.10, ** p<0.05, *** p<0.01

Table A.21: The Effect of Free Early Education Places on **Writing L3+**

Dependent Variable: Percentage of children in the LEA's maintained school achieving Level 3 or higher in Key Stage 1 assessment, year (*t+3*).

Panel Regression Models: 4 year panel

	sample 1		**sample 2**		**sample 3**	
Independent Variable	Coeff.	Rob. St. Err.	Coeff.	Rob. St. Err.	Coeff.	Rob. St. Err.
3-year olds EE take-up, sch sector 'poorer' LEAs (t)	0.016	0.039	0.026	0.043	0.023	0.036
3-year olds EE take-up, sch sector 'better-off' LEAs (t)	-0.011	0.028	-0.005	0.029	-0.012	0.030
3-year olds EE take-up, 'other' sector 'poorer' LEAs (t)	-0.016	0.015	-0.010	0.013	-0.008	0.013
3-year olds EE take-up, 'other' sector 'better-off' LEAs (t)	0.011	0.009	0.010	0.009	0.010	0.008
4-year olds EE take-up, sch sector 'poorer' LEAs (t+1)	-0.008	0.069	-0.021	0.064	-0.017	0.061
4-year olds EE take-up, sch sector 'better-off' LEAs (t+1)	**0.157****	0.077	**0.128***	0.076	**0.139****	0.063
4-year olds EE take-up, 'other' sector 'poorer' LEAs (t+1)	-0.020	0.045	-0.020	0.043	-0.029	0.044
4-year olds EE take-up, 'other' sector 'better-off' LEAs (t+1)	-0.001	0.039	-0.002	0.038	0.020	0.032
childcare places available: day nursery (t)	0.029	0.044	0.025	0.042	0.030	0.036
childcare places available: childminder (t)	-0.034	0.063	-0.052	0.062	-0.068	0.056
childcare places available: playgroup (t)	**-0.039***	0.023	**-0.038***	0.022	**-0.041****	0.019
manufacturing jobs rate (t)	**0.082****	0.041	**0.081****	0.040	**0.076****	0.037
average KS1 class size (t+3)	**-0.45****	0.174	**-0.418****	0.167	**-0.343****	0.159
year=2003	**6.890*****	0.33	**6.804*****	0.305	**6.835*****	0.279
year=2004	**6.582*****	0.406	**6.469*****	0.379	**6.511*****	0.352
year=2006	**4.927*****	0.598	**4.847*****	0.559	**4.776*****	0.524
constant	**13.768***	7.005	**14.347****	6.461	**11.946****	5.799
R^2 within	0.779		0.775		0.781	
R^2 between	0.321		0.271		0.245	
R^2 overall	0.396		0.384		0.354	
ρ	0.894		0.869		0.886	
#obs	464		503		566	
#groups	120		130		148	
LEA level fixed effects included	Yes		Yes		Yes	

ρ is the fraction of the variance due to the fixed effects

* p<0.10, ** p<0.05, *** p<0.01

Table A.22: The Effect of Free Early Education Places on **Maths L3+**

Dependent Variable: Percentage of children in the LEA's maintained school achieving Level 3 or higher in Key Stage 1 assessment, year (*t+3*).

Panel Regression Models: 4 year panel

	sample 1		**sample 2**		**sample 3**	
Independent Variable	Coeff.	Rob. St. Err.	Coeff.	Rob. St. Err.	Coeff.	Rob. St. Err.
3-year olds EE take-up, sch sector 'poorer' LEAs (t)	0.016	0.043	0.041	0.040	0.038	0.032
3-year olds EE take-up, sch sector 'better-off' LEAs (t)	-0.051	0.039	-0.046	0.039	-0.035	0.039
3-year olds EE take-up, 'other' sector 'poorer' LEAs (t)	**-0.025**†	0.015	-0.018	0.012	-0.014	0.012
3-year olds EE take-up, 'other' sector 'better-off' LEAs (t)	-0.003	0.013	-0.003	0.012	-0.002	0.011
4-year olds EE take-up, sch sector 'poorer' LEAs (t+1)	-0.014	0.061	-0.01	0.053	-0.005	0.051
4-year olds EE take-up, sch sector 'better-off' LEAs (t+1)	**0.155****	0.072	**0.134***	0.069	0.094	0.068
4-year olds EE take-up, 'other' sector 'poorer' LEAs (t+1)	0.029	0.041	0.01	0.039	-0.008	0.040
4-year olds EE take-up, 'other' sector 'better-off' LEAs (t+1)	-0.020	0.047	-0.026	0.044	-0.032	0.037
childcare places available: day nursery (t)	0.010	0.050	0.006	0.050	0.027	0.045
childcare places available: childminder (t)	0.050	0.069	0.037	0.065	0.047	0.058
childcare places available: playgroup (t)	-0.007	0.029	0.002	0.026	-0.001	0.024
economic inactivity rate, working age males (t)	0.028	0.036	0.03	0.035	0.036	0.032
median weekly gross pay, male f-t workers (t)	0.005	0.006	0.005	0.006	0.007	0.006
average KS1 class size (t+3)	**-0.726*****	0.170	**-0.632*****	0.163	**-0.467****	0.183
% of non-white children (t+3)	0.005	0.086	0.023	0.081	0.059	0.076
year=2003	**-1.869*****	0.307	**-2.051*****	0.296	**-2.203*****	0.281
year=2004	**-2.819*****	0.432	**-2.938*****	0.406	**-3.038*****	0.382
year=2006	**-9.270*****	0.671	**-9.396*****	0.642	**-9.896*****	0.622
constant	**40.709*****	6.550	**37.987*****	6.320	**33.702*****	6.047
R^2 within	0.850		0.851		0.849	
R^2 between	0.427		0.312		0.06	
R^2 overall	0.575		0.544		0.395	
ρ	0.825		0.809		0.846	
#obs	463		502		565	
#groups	120		130		148	
LEA level fixed effects included	Yes		Yes		Yes	

ρ is the fraction of the variance due to the fixed effects

* $p<0.10$, ** $p<0.05$, *** $p<0.01$, † $p=0.102$

A.5 Robustness: Models #1 & #2, Free Places Only

The panel used for the main estimation is a 5-year panel, with results data annually from the years 2001 to 2006, though with data from 2005 missing due to the non-availability of childcare place provision variables in 2002. Consequently, if I exclude the childcare availability variables from the model, this allows the free early education place take-up data from 2002 (and results from year 2005) to be utilised – thus making it a 6-year panel. I do this as a robustness check of the results – to show that adding in an extra year of data, and removing the childcare control variables, does not qualitatively or quantitatively alter the nature of the results. Tables A.23 to A.29 report the results of various model estimations when I exclude the childcare variables and therefore extend the panel to be 6 years for the level 2B or higher results estimations. For the level 3 or higher results estimations, excluding the childcare variables extends the panel from 4 years to 5 years.

Results are shown not only for the main estimation sample but also for samples 2 and 3 that are included as robustness checks of the main results.

Table A.23: The Effect of Free Early Education Places on **Reading L2B+** Model #1

Dependent Variable: Percentage of children in the LEA's maintained school achieving Level 2B or higher in Key Stage 1 assessment, year (*t+3*).

Panel Regression Models: 6 year panel

	sample 1		**sample 2**		**sample 3**	
Independent Variable	Coeff.	Rob. St. Err.	Coeff.	Rob. St. Err.	Coeff.	Rob. St. Err.
3-year olds early educ take-up, school sector (t)	**0.027***	0.014	**0.036****	0.016	**0.037****	0.016
3-year olds early educ take-up, 'other' sector (t)	0.005	0.007	-0.001	0.007	0.001	0.007
4-year olds early educ take-up, school sector (t+1)	-0.012	0.043	-0.021	0.042	-0.027	0.040
4-year olds early educ take-up, 'other' sector (t+1)	-0.014	0.023	-0.018	0.022	-0.010	0.021
average KS1 class size (t+3)	**-0.419*****	0.154	**-0.360****	0.150	**-0.343****	0.139
year=2002	-0.101	0.160	-0.111	0.160	-0.021	0.156
year=2003	0.208	0.199	0.217	0.200	0.246	0.189
year=2004	**1.854*****	0.255	**1.912*****	0.275	**1.891*****	0.239
year=2005	**3.617*****	0.298	**3.746*****	0.291	**3.682*****	0.278
year=2006	**2.776*****	0.379	**2.923*****	0.378	**2.873*****	0.359
constant	**78.611*****	5.181	**77.468*****	5.137	**77.365*****	4.789
R^2 within	0.532		0.533		0.533	
R^2 between	0.043		0.076		0.068	
R^2 overall	0.024		0.010		0.016	
ρ	0.919		0.921		0.917	
#obs	708		758		858	
#groups	120		130		148	
LEA level fixed effects included	Yes		Yes		Yes	

ρ is the fraction of the variance due to the fixed effects

* p<0.10, ** p<0.05, *** p<0.01

Table A.24: The Effect of Free Early Education Places on **Reading L2B+** Model #2

Dependent Variable: Percentage of children in the LEA's maintained school achieving Level 2B or higher in Key Stage 1 assessment, year (*t+3*).

Panel Regression Models: 6 year panel

	sample 1		sample 2		sample 3	
Independent Variable	Coeff.	Rob. St. Err.	Coeff.	Rob. St. Err.	Coeff.	Rob. St. Err.
3-year olds EE take-up, sch sector 'poorer' LEAs (t)	**0.044***	0.024	**0.054****	0.024	**0.047***	0.026
3-year olds EE take-up, sch sector 'better-off' LEAs (t)	0.022	0.017	0.030	0.018	**0.033***	0.018
3-year olds EE take-up, 'other' sector 'poorer' LEAs (t)	0.002	0.015	-0.001	0.013	0.000	0.012
3-year olds EE take-up, 'other' sector 'better-off' LEAs (t)	0.006	0.007	-0.001	0.007	0.002	0.007
4-year olds EE take-up, sch sector 'poorer' LEAs (t+1)	0.007	0.051	0.002	0.048	-0.009	0.047
4-year olds EE take-up, sch sector 'better-off' LEAs (t+1)	-0.032	0.065	-0.056	0.070	-0.047	0.063
4-year olds EE take-up, 'other' sector 'poorer' LEAs (t+1)	-0.041	0.054	-0.032	0.046	-0.031	0.044
4-year olds EE take-up, 'other' sector 'better-off' LEAs (t+1)	0.004	0.029	-0.011	0.028	0.002	0.027
economic inactivity rate, working age males (t)	-0.019	0.024	-0.020	0.024	-0.017	0.022
manufacturing jobs rate (t)	-0.036	0.025	-0.022	0.023	-0.017	0.022
average KS1 class size (t+3)	**-0.424*****	0.157	**-0.357****	0.149	**-0.348****	0.138
year=2002	-0.169	0.171	-0.167	0.172	-0.069	0.170
year=2003	0.168	0.219	0.153	0.211	0.225	0.203
year=2004	**1.796*****	0.286	**1.841*****	0.268	**1.873*****	0.260
year=2005	**3.552*****	0.330	**3.674*****	0.310	**3.669*****	0.298
year=2006	**2.693*****	0.418	**2.829*****	0.403	**2.846*****	0.386
constant	**79.237*****	5.104	**78.184*****	5.070	**78.042*****	4.739
R^2 within	0.546		0.537		0.535	
R^2 between	0.324		0.338		0.284	
R^2 overall	0.089		0.151		0.065	
ρ	0.949		0.959		0.944	
#obs	708		758		858	
#groups	120		130		148	
LEA level fixed effects included	Yes		Yes		Yes	

ρ is the fraction of the variance due to the fixed effects

* p<0.10, ** p<0.05, *** p<0.01

Table A.25: The Effect of Free Early Education Places on **Writing L2B+** Model #2

Dependent Variable: Percentage of children in the LEA's maintained school achieving Level 2B or higher in Key Stage 1 assessment, year (*t+3*).

Panel Regression Models: 6 year panel

	sample 1		sample 2		sample 3	
Independent Variable	Coeff.	Rob. St. Err.	Coeff.	Rob. St. Err.	Coeff.	Rob. St. Err.
3-year olds EE take-up, sch sector 'poorer' LEAs (t)	**0.065****	0.029	**0.065****	0.028	**0.058****	0.024
3-year olds EE take-up, sch sector 'better-off' LEAs (t)	-0.011	0.017	-0.002	0.020	-0.006	0.023
3-year olds EE take-up, 'other' sector 'poorer' LEAs (t)	0.012	0.019	0.013	0.017	0.017	0.017
3-year olds EE take-up, 'other' sector 'better-off' LEAs (t)	0.016	0.010	0.012	0.010	0.012	0.009
4-year olds EE take-up, sch sector 'poorer' LEAs (t+1)	-0.074	0.055	-0.054	0.051	-0.058	0.049
4-year olds EE take-up, sch sector 'better-off' LEAs (t+1)	-0.007	0.068	-0.041	0.076	-0.036	0.067
4-year olds EE take-up, 'other' sector 'poorer' LEAs (t+1)	-0.040	0.074	-0.036	0.062	-0.050	0.060
4-year olds EE take-up, 'other' sector 'better-off' LEAs (t+1)	0.023	0.032	0.005	0.032	0.018	0.027
economic inactivity rate, working age males (t)	-0.013	0.028	-0.012	0.027	-0.004	0.025
manufacturing jobs rate (t)	-0.057	0.035	-0.036	0.033	-0.019	0.032
average KS1 class size (t+3)	**-0.605*****	0.213	**-0.585*****	0.206	**-0.532*****	0.194
year=2002	**0.795*****	0.219	**0.811*****	0.217	**0.841*****	0.208
year=2003	**3.338*****	0.321	**3.336*****	0.307	**3.367*****	0.278
year=2004	**2.861*****	0.364	**2.900*****	0.346	**2.988*****	0.323
year=2005	**2.833*****	0.459	**2.927*****	0.438	**3.063*****	0.400
year=2006	**1.708*****	0.563	**1.784*****	0.549	**1.928*****	0.510
constant	**75.986*****	5.924	**75.505*****	6.122	**73.866*****	5.817
R^2 within	0.376		0.363		0.369	
R^2 between	0.284		0.156		0.006	
R^2 overall	0.289		0.008		0.022	
ρ	0.850		0.903		0.886	
#obs	708		758		858	
#groups	120		130		148	
LEA level fixed effects included	Yes		Yes		Yes	

ρ is the fraction of the variance due to the fixed effects

* p<0.10, ** p<0.05, *** p<0.01

Table A.26: The Effect of Free Early Education Places on **Maths L2B+** Model #2

Dependent Variable: Percentage of children in the LEA's maintained school achieving Level 2B or higher in Key Stage 1 assessment, year (*t+3*).

Panel Regression Models: 6 year panel

	sample 1		sample 2		sample 3	
Independent Variable	Coeff.	Rob. St. Err.	Coeff.	Rob. St. Err.	Coeff.	Rob. St. Err.
3-year olds EE take-up, sch sector 'poorer' LEAs (t)	-0.016	0.036	-0.006	0.037	-0.005	0.036
3-year olds EE take-up, sch sector 'better-off' LEAs (t)	-0.012	0.028	-0.005	0.030	0.007	0.034
3-year olds EE take-up, 'other' sector 'poorer' LEAs (t)	-0.002	0.016	0.000	0.013	-0.004	0.012
3-year olds EE take-up, 'other' sector 'better-off' LEAs (t)	**0.016****	0.008	0.010	0.008	0.010	0.008
4-year olds EE take-up, sch sector 'poorer' LEAs (t+1)	-0.040	0.052	-0.024	0.049	-0.032	0.051
4-year olds EE take-up, sch sector 'better-off' LEAs (t+1)	0.027	0.048	0.003	0.055	0.010	0.051
4-year olds EE take-up, 'other' sector 'poorer' LEAs (t+1)	-0.020	0.064	-0.041	0.054	-0.047	0.052
4-year olds EE take-up, 'other' sector 'better-off' LEAs (t+1)	-0.007	0.024	-0.015	0.024	-0.016	0.020
economic inactivity rate, working age males (t)	0.012	0.021	0.015	0.021	0.002	0.021
manufacturing jobs rate (t)	-0.033	0.029	-0.012	0.029	-0.011	0.027
average KS1 class size (t+3)	-0.233	0.159	-0.167	0.161	-0.151	0.153
% of non-white children (t+3)	**-0.122****	0.054	**-0.123****	0.054	**-0.090***	0.050
year=2002	0.002	0.170	0.022	0.168	-0.003	0.156
year=2003	**-1.962*****	0.226	**-1.995*****	0.224	**-2.037*****	0.210
year=2004	-0.006	0.303	0.060	0.298	0.139	0.285
year=2005	**-1.236*****	0.385	**-1.120*****	0.374	**-1.209*****	0.364
year=2006	**-2.313*****	0.460	**-2.203*****	0.453	**-2.231*****	0.435
constant	**84.531*****	4.697	**82.427*****	5.017	**81.506*****	4.721
R^2 within	0.403		0.393		0.387	
R^2 between	0.632		0.581		0.564	
R^2 overall	0.597		0.559		0.543	
ρ	0.796		0.770		0.768	
#obs	707		757		857	
#groups	120		130		148	
LEA level fixed effects included	Yes		Yes		Yes	

ρ is the fraction of the variance due to the fixed effects

* p<0.10, ** p<0.05, *** p<0.01

Table A.27: The Effect of Free Early Education Places on **Reading L3+** Model #2

Dependent Variable: Percentage of children in the LEA's maintained school achieving Level 3 or higher in Key Stage 1 assessment, year (*t+3*).

Panel Regression Models: 5 year panel

	sample 1		**sample 2**		**sample 3**	
Independent Variable	Coeff.	Rob. St. Err.	Coeff.	Rob. St. Err.	Coeff.	Rob. St. Err.
3-year olds EE take-up, sch sector 'poorer' LEAs (t)	0.041	0.045	**0.072***	0.042	0.050	0.038
3-year olds EE take-up, sch sector 'better-off' LEAs (t)	-0.025	0.031	-0.014	0.031	-0.004	0.031
3-year olds EE take-up, 'other' sector 'poorer' LEAs (t)	0.008	0.011	0.012	0.009	0.010	0.009
3-year olds EE take-up, 'other' sector 'better-off' LEAs (t)	0.002	0.009	-0.001	0.009	0.000	0.008
4-year olds EE take-up, sch sector 'poorer' LEAs (t+1)	-0.051	0.051	-0.057	0.045	-0.051	0.041
4-year olds EE take-up, sch sector 'better-off' LEAs (t+1)	**0.130****	0.058	0.089	0.065	0.065	0.062
4-year olds EE take-up, 'other' sector 'poorer' LEAs (t+1)	-0.013	0.041	-0.021	0.036	-0.034	0.035
4-year olds EE take-up, 'other' sector 'better-off' LEAs (t+1)	0.021	0.033	0.006	0.032	-0.001	0.028
economic inactivity rate, working age males (t)	-0.004	0.024	-0.012	0.023	-0.005	0.021
median weekly gross pay, male f-t workers (t)	0.004	0.004	0.006	0.004	**0.007***	0.004
average KS1 class size (t+3)	**-0.511*****	0.150	**-0.434*****	0.144	**-0.362****	0.147
year=2003	**-2.035*****	0.169	**-2.155*****	0.161	**-2.093*****	0.155
year=2004	**-1.514*****	0.253	**-1.617*****	0.239	**-1.452*****	0.233
year=2005	**-3.036*****	0.347	**-3.108*****	0.324	**-3.089*****	0.305
year=2006	**-4.195*****	0.445	**-4.300*****	0.419	**-4.287*****	0.393
constant	**36.333*****	5.332	**34.897*****	5.302	**33.465*****	4.856
R^2 within	0.589		0.580		0.554	
R^2 between	0.458		0.381		0.331	
R^2 overall	0.466		0.408		0.361	
ρ	0.908		0.885		0.883	
#obs	589		639		727	
#groups	120		130		148	
LEA level fixed effects included	Yes		Yes		Yes	

ρ is the fraction of the variance due to the fixed effects

* p<0.10, ** p<0.05, *** p<0.01

Table A.28: The Effect of Free Early Education Places on **Writing L3+** Model #2

Dependent Variable: Percentage of children in the LEA's maintained school achieving Level 3 or higher in Key Stage 1 assessment, year (*t+3*).

Panel Regression Models: 5 year panel

	sample 1		sample 2		sample 3	
Independent Variable	Coeff.	Rob. St. Err.	Coeff.	Rob. St. Err.	Coeff.	Rob. St. Err.
3-year olds EE take-up, sch sector 'poorer' LEAs (t)	0.005	0.035	0.005	0.034	0.012	0.029
3-year olds EE take-up, sch sector 'better-off' LEAs (t)	-0.015	0.028	-0.008	0.028	-0.014	0.029
3-year olds EE take-up, 'other' sector 'poorer' LEAs (t)	-0.001	0.018	0.004	0.015	0.004	0.014
3-year olds EE take-up, 'other' sector 'better-off' LEAs (t)	0.011	0.008	0.011	0.008	0.009	0.008
4-year olds EE take-up, sch sector 'poorer' LEAs (t+1)	-0.003	0.056	-0.014	0.055	-0.015	0.052
4-year olds EE take-up, sch sector 'better-off' LEAs (t+1)	**0.105***	0.062	0.068	0.066	0.077	0.055
4-year olds EE take-up, 'other' sector 'poorer' LEAs (t+1)	**-0.101***	0.061	**-0.096***	0.052	**-0.098***	0.051
4-year olds EE take-up, 'other' sector 'better-off' LEAs (t+1)	-0.006	0.043	-0.012	0.039	0.003	0.032
manufacturing jobs rate (t)	0.045	0.032	0.045	0.029	0.045	0.028
average KS1 class size (t+3)	-0.148	0.157	-0.159	0.148	-0.108	0.142
year=2003	**6.745*****	0.343	**6.675*****	0.313	**6.756*****	0.281
year=2004	**6.438*****	0.391	**6.344*****	0.358	**6.444*****	0.331
year=2005	**5.933*****	0.468	**5.827*****	0.427	**5.889*****	0.391
year=2006	**4.849*****	0.528	**4.762*****	0.485	**4.810*****	0.449
constant	8.277	5.450	**10.296***	5.222	**8.285***	4.814
R^2 within	0.736		0.734		0.741	
R^2 between	0.321		0.273		0.254	
R^2 overall	0.425		0.423		0.402	
ρ	0.834		0.799		0.816	
#obs	589		639		727	
#groups	120		130		148	
LEA level fixed effects included	Yes		Yes		Yes	

ρ is the fraction of the variance due to the fixed effects

* p<0.10, ** p<0.05, *** p<0.01

Table A.29: The Effect of Free Early Education Places on **Maths L3+** Model #2

Dependent Variable: Percentage of children in the LEA's maintained school achieving Level 3 or higher in Key Stage 1 assessment, year (*t+3*).

Panel Regression Models: 5 year panel

	sample 1		**sample 2**		**sample 3**	
Independent Variable	Coeff.	Rob. St. Err.	Coeff.	Rob. St. Err.	Coeff.	Rob. St. Err.
3-year olds EE take-up, sch sector 'poorer' LEAs (t)	0.026	0.044	0.040	0.037	0.034	0.030
3-year olds EE take-up, sch sector 'better-off' LEAs (t)	-0.042	0.030	-0.038	0.031	-0.031	0.030
3-year olds EE take-up, 'other' sector 'poorer' LEAs (t)	-0.009	0.016	-0.006	0.013	-0.005	0.012
3-year olds EE take-up, 'other' sector 'better-off' LEAs (t)	-0.003	0.011	-0.003	0.010	-0.002	0.009
4-year olds EE take-up, sch sector 'poorer' LEAs (t+1)	-0.023	0.055	-0.017	0.049	-0.011	0.046
4-year olds EE take-up, sch sector 'better-off' LEAs (t+1)	**0.165*** **	0.061	**0.140****	0.061	**0.118***	0.060
4-year olds EE take-up, 'other' sector 'poorer' LEAs (t+1)	-0.016	0.053	-0.022	0.044	-0.033	0.042
4-year olds EE take-up, 'other' sector 'better-off' LEAs (t+1)	0.014	0.036	0.002	0.032	-0.006	0.028
economic inactivity rate, working age males (t)	0.026	0.028	0.031	0.027	0.032	0.024
median weekly gross pay, male f-t workers (t)	0.002	0.005	0.001	0.005	0.004	0.004
average KS1 class size (t+3)	**-0.458*****	0.172	**-0.407****	0.161	**-0.310***	0.172
% of non-white children (t+3)	-0.020	0.084	0.003	0.080	0.029	0.075
year=2003	**-1.912*****	0.325	**-2.083*****	0.306	**-2.174*****	0.284
year=2004	**-2.797*****	0.436	**-2.920*****	0.407	**-2.908*****	0.376
year=2005	**-8.019*****	0.577	**-8.153*****	0.538	**-8.377*****	0.496
year=2006	**-9.204*****	0.664	**-9.359*****	0.624	**-9.627*****	0.580
constant	**35.509*****	6.000	**34.319*****	5.777	**31.254*****	5.607
R^2 within	0.849		0.852		0.848	
R^2 between	0.441		0.349		0.226	
R^2 overall	0.551		0.543		0.487	
ρ	0.872		0.836		0.838	
#obs	588		638		726	
#groups	120		130		148	
LEA level fixed effects included	Yes		Yes		Yes	

ρ is the fraction of the variance due to the fixed effects

* p<0.10, ** p<0.05, *** p<0.01

A.6 Robustness: Model #2, Excluding 2006

The Tables A.30 to A.35 below report the regression results for the second specification of the model looking at actual take-up rates of free early education places in each type of setting i.e. the specification allowing different effects of take-up in the different types of LEA. The results are reported for each subject at each level for each sample, but with the data from results year 2006 excluded, to confirm that the results still hold when we exclude this data. I run this robustness check because for the years 2001 to 2004, the published results refer to the children's attainments in a standard national task/test, thus this is a consistent dependent variable. From 2005 onwards however, the assessment altered slightly to be a teacher assessed level for the child – based on their performance in the standard national task/test but also taking into account the teacher's own knowledge of the child. Clearly this is something that could potentially affect results, and could affect things differentially across LEAs depending on the teachers' attitudes within each LEA. In light of the potential problems caused by this alteration in assessment method, I robustness check the results by running the regressions both with and without 2006 results data included.

Table A.30: The Effect of Free Early Education Places on **Reading L2B+** Model #2

Dependent Variable: Percentage of children in the LEA's maintained school achieving Level 2B or higher in Key Stage 1 assessment, year (*t+3*). Data from 2006 excluded

Panel Regression Models: 4 year panel

	sample 1		**sample 2**		**sample 3**	
Independent Variable	Coeff.	Rob. St. Err.	Coeff.	Rob. St. Err.	Coeff.	Rob. St. Err.
3-year olds EE take-up, sch sector 'poorer' LEAs (t)	**0.080*** **	0.028	**0.073*** **	0.027	**0.072*** **	0.027
3-year olds EE take-up, sch sector 'better-off' LEAs (t)	-0.020	0.034	-0.017	0.035	-0.015	0.034
3-year olds EE take-up, 'other' sector 'poorer' LEAs (t)	-0.010	0.014	-0.010	0.012	-0.011	0.012
3-year olds EE take-up, 'other' sector 'better-off' LEAs (t)	0.000	0.011	-0.006	0.010	-0.006	0.009
4-year olds EE take-up, sch sector 'poorer' LEAs (t+1)	-0.075	0.060	-0.074	0.054	-0.079	0.054
4-year olds EE take-up, sch sector 'better-off' LEAs (t+1)	-0.055	0.066	-0.068	0.066	-0.074	0.062
4-year olds EE take-up, 'other' sector 'poorer' LEAs (t+1)	-0.012	0.045	-0.001	0.045	0.004	0.045
4-year olds EE take-up, 'other' sector 'better-off' LEAs (t+1)	-0.014	0.033	-0.023	0.033	-0.002	0.034
childcare places available: day nursery (t)	-0.023	0.042	-0.024	0.041	-0.018	0.037
childcare places available: childminder (t)	-0.005	0.059	-0.001	0.055	0.024	0.052
childcare places available: playgroup (t)	-0.006	0.020	-0.011	0.020	-0.016	0.019
economic inactivity rate, working age males (t)	**-0.081*** **	0.029	**-0.072** **	0.029	**-0.058** **	0.028
manufacturing jobs rate (t)	-0.044	0.028	**-0.050* **	0.027	**-0.046* **	0.025
average KS1 class size (t+3)	-0.208	0.229	-0.230	0.213	-0.232	0.192
year=2002	-0.166	0.168	-0.153	0.170	-0.158	0.168
year=2003	0.151	0.255	0.138	0.241	0.153	0.226
year=2004	**1.885*** **	0.344	**1.919*** **	0.318	**1.900*** **	0.301
constant	**79.305*** **	6.546	**80.487*** **	6.388	**80.317*** **	6.001
R^2 within	0.353		0.355		0.346	
R^2 between	0.232		0.293		0.206	
R^2 overall	0.112		0.170		0.117	
ρ	0.948		0.954		0.950	
#obs	456		485		532	
#groups	120		130		148	
LEA level fixed effects included	Yes		Yes		Yes	

ρ is the fraction of the variance due to the fixed effects

* p<0.10, ** p<0.05, *** p<0.01

Table A.31: The Effect of Free Early Education Places on **Writing L2B+** Model #2

Dependent Variable: Percentage of children in the LEA's maintained school achieving Level 2B or higher in Key Stage 1 assessment, year (*t+3*). Data from 2006 excluded

Panel Regression Models: 4 year panel

	sample 1		**sample 2**		**sample 3**	
Independent Variable	Coeff.	Rob. St. Err.	Coeff.	Rob. St. Err.	Coeff.	Rob. St. Err.
3-year olds EE take-up, sch sector 'poorer' LEAs (t)	**0.070***	0.037	0.057	0.036	**0.062***	0.035
3-year olds EE take-up, sch sector 'better-off' LEAs (t)	-0.004	0.052	0.001	0.054	0.015	0.053
3-year olds EE take-up, 'other' sector 'poorer' LEAs (t)	-0.003	0.017	0.003	0.016	0.003	0.016
3-year olds EE take-up, 'other' sector 'better-off' LEAs (t)	-0.013	0.019	-0.012	0.017	-0.012	0.015
4-year olds EE take-up, sch sector 'poorer' LEAs (t+1)	-0.080	0.072	-0.058	0.068	-0.063	0.065
4-year olds EE take-up, sch sector 'better-off' LEAs (t+1)	-0.035	0.075	-0.047	0.075	-0.054	0.068
4-year olds EE take-up, 'other' sector 'poorer' LEAs (t+1)	-0.035	0.062	-0.015	0.064	-0.012	0.065
4-year olds EE take-up, 'other' sector 'better-off' LEAs (t+1)	0.007	0.043	0.001	0.041	0.033	0.040
childcare places available: day nursery (t)	-0.048	0.057	-0.041	0.055	-0.028	0.050
childcare places available: childminder (t)	**-0.114***	0.068	-0.082	0.066	-0.051	0.062
childcare places available: playgroup (t)	-0.001	0.023	-0.012	0.025	-0.015	0.024
economic inactivity rate, working age males (t)	-0.049	0.032	-0.036	0.032	-0.026	0.031
manufacturing jobs rate (t)	-0.061	0.043	-0.063	0.041	-0.054	0.040
average KS1 class size (t+3)	**-0.671***	0.339	**-0.826****	0.325	**-0.759****	0.306
year=2002	**0.833*****	0.239	**0.826*****	0.235	**0.757*****	0.226
year=2003	**3.423*****	0.381	**3.400*****	0.363	**3.365*****	0.337
year=2004	**3.333*****	0.488	**3.289*****	0.461	**3.281*****	0.432
constant	**81.032*****	8.663	**84.429*****	8.553	**81.971*****	8.258
R^2 within	0.485		0.484		0.476	
R^2 between	0.019		0.121		0.045	
R^2 overall	0.080		0.002		0.002	
ρ	0.895		0.918		0.910	
#obs	456		485		532	
#groups	120		130		148	
LEA level fixed effects included	Yes		Yes		Yes	

ρ is the fraction of the variance due to the fixed effects

* p<0.10, ** p<0.05, *** p<0.01

Table A.32: The Effect of Free Early Education Places on **Maths L2B+** Model #2

Dependent Variable: Percentage of children in the LEA's maintained school achieving Level 2B or higher in Key Stage 1 assessment, year (*t+3*). Data from 2006 excluded

Panel Regression Models: 4 year panel

	sample 1		sample 2		sample 3	
Independent Variable	Coeff.	Rob. St. Err.	Coeff.	Rob. St. Err.	Coeff.	Rob. St. Err.
3-year olds EE take-up, sch sector 'poorer' LEAs (t)	**-0.076*** **	0.029	**-0.071** **	0.030	**-0.070** **	0.029
3-year olds EE take-up, sch sector 'better-off' LEAs (t)	-0.049	0.043	-0.044	0.043	-0.029	0.047
3-year olds EE take-up, 'other' sector 'poorer' LEAs (t)	**-0.027** **	0.013	-0.020	0.013	-0.020	0.012
3-year olds EE take-up, 'other' sector 'better-off' LEAs (t)	-0.002	0.010	-0.005	0.009	-0.002	0.009
4-year olds EE take-up, sch sector 'poorer' LEAs (t+1)	0.011	0.050	-0.004	0.050	0.007	0.048
4-year olds EE take-up, sch sector 'better-off' LEAs (t+1)	0.038	0.045	0.022	0.045	0.015	0.040
4-year olds EE take-up, 'other' sector 'poorer' LEAs (t+1)	0.020	0.045	0.023	0.046	0.016	0.045
4-year olds EE take-up, 'other' sector 'better-off' LEAs (t+1)	-0.029	0.028	-0.025	0.028	-0.031	0.025
childcare places available: day nursery (t)	-0.034	0.047	-0.034	0.044	-0.013	0.040
childcare places available: childminder (t)	0.040	0.048	0.039	0.045	0.049	0.039
childcare places available: playgroup (t)	-0.024	0.021	-0.026	0.020	-0.022	0.020
economic inactivity rate, working age males (t)	-0.007	0.023	0.008	0.024	0.004	0.024
manufacturing jobs rate (t)	-0.025	0.033	-0.026	0.032	-0.019	0.031
average KS1 class size (t+3)	-0.213	0.228	-0.212	0.213	-0.061	0.208
% of non-white children (t+3)	**-0.098***	0.051	**-0.101** **	0.051	**-0.085***	0.049
year=2002	0.098	0.185	0.084	0.180	0.025	0.173
year=2003	**-1.681*** **	0.265	**-1.789*** **	0.253	**-1.931*** **	0.248
year=2004	0.452	0.393	0.380	0.374	0.303	0.360
constant	**83.986*** **	5.911	**84.594*** **	5.716	**79.916*** **	5.730
R^2 within	0.440		0.444		0.446	
R^2 between	0.589		0.552		0.533	
R^2 overall	0.572		0.546		0.532	
ρ	0.814		0.824		0.827	
#obs	455		484		531	
#groups	120		130		148	
LEA level fixed effects included	Yes		Yes		Yes	

ρ is the fraction of the variance due to the fixed effects

* p<0.10, ** p<0.05, *** p<0.01

Table A.33: The Effect of Free Early Education Places on **Reading L3+** Model #2

Dependent Variable: Percentage of children in the LEA's maintained school achieving Level 3 or higher in Key Stage 1 assessment, year (*t+3*). Data from 2006 excluded

Panel Regression Models: 3 year panel

	sample 1		**sample 2**		**sample 3**	
Independent Variable	Coeff.	Rob. St. Err.	Coeff.	Rob. St. Err.	Coeff.	Rob. St. Err.
3-year olds EE take-up, sch sector 'poorer' LEAs (t)	-0.062	0.055	-0.025	0.050	-0.047	0.047
3-year olds EE take-up, sch sector 'better-off' LEAs (t)	**-0.100*****	0.032	**-0.094*****	0.031	-0.046	0.049
3-year olds EE take-up, 'other' sector 'poorer' LEAs (t)	-0.002	0.012	0.004	0.010	0.005	0.010
3-year olds EE take-up, 'other' sector 'better-off' LEAs (t)	0.002	0.010	0.002	0.009	0.005	0.009
4-year olds EE take-up, sch sector 'poorer' LEAs (t+1)	0.031	0.062	-0.011	0.057	-0.023	0.051
4-year olds EE take-up, sch sector 'better-off' LEAs (t+1)	**0.167*****	0.057	**0.155*****	0.057	0.094	0.067
4-year olds EE take-up, 'other' sector 'poorer' LEAs (t+1)	0.007	0.045	0.009	0.043	-0.009	0.041
4-year olds EE take-up, 'other' sector 'better-off' LEAs (t+1)	0.042	0.047	0.045	0.044	0.045	0.036
childcare places available: day nursery (t)	-0.043	0.041	-0.027	0.040	0.009	0.040
childcare places available: childminder (t)	-0.075	0.046	**-0.089***	0.047	-0.056	0.039
childcare places available: playgroup (t)	0.010	0.017	0.014	0.015	0.011	0.016
economic inactivity rate, working age males (t)	-0.014	0.026	-0.014	0.024	-0.004	0.027
median weekly gross pay, male f-t workers (t)	0.007	0.007	0.006	0.007	0.008	0.006
average KS1 class size (t+3)	**-0.604*****	0.221	**-0.498****	0.214	-0.226	0.221
year=2003	**-1.853*****	0.215	**-1.953*****	0.209	**-2.031*****	0.200
year=2004	**-1.363*****	0.316	**-1.459*****	0.302	**-1.544*****	0.286
constant	**37.361*****	6.997	**35.885*****	6.820	**30.073*****	6.519
R^2 within	0.528		0.533		0.465	
R^2 between	0.466		0.421		0.383	
R^2 overall	0.468		0.421		0.383	
ρ	0.938		0.949		0.942	
#obs	345		374		419	
#groups	120		130		148	
LEA level fixed effects included	Yes		Yes		Yes	

ρ is the fraction of the variance due to the fixed effects

* p<0.10, ** p<0.05, *** p<0.01

Table A.34: The Effect of Free Early Education Places on **Writing L3+** Model #2

Dependent Variable: Percentage of children in the LEA's maintained school achieving Level 3 or higher in Key Stage 1 assessment, year (*t+3*). Data from 2006 excluded

Panel Regression Models: 3 year panel

	sample 1		sample 2		sample 3	
Independent Variable	Coeff.	Rob. St. Err.	Coeff.	Rob. St. Err.	Coeff.	Rob. St. Err.
3-year olds EE take-up, sch sector 'poorer' LEAs (t)	0.030	0.065	0.009	0.071	0.019	0.063
3-year olds EE take-up, sch sector 'better-off' LEAs (t)	-0.014	0.043	-0.010	0.042	-0.001	0.046
3-year olds EE take-up, 'other' sector 'poorer' LEAs (t)	**-0.026***	0.014	-0.019	0.014	-0.018	0.014
3-year olds EE take-up, 'other' sector 'better-off' LEAs (t)	0.004	0.011	0.010	0.010	0.012	0.009
4-year olds EE take-up, sch sector 'poorer' LEAs (t+1)	-0.056	0.076	-0.065	0.071	-0.061	0.066
4-year olds EE take-up, sch sector 'better-off' LEAs (t+1)	**0.163****	0.070	**0.158****	0.067	**0.156****	0.066
4-year olds EE take-up, 'other' sector 'poorer' LEAs (t+1)	-0.021	0.044	-0.003	0.044	-0.017	0.044
4-year olds EE take-up, 'other' sector 'better-off' LEAs (t+1)	0.026	0.051	0.016	0.048	0.036	0.039
childcare places available: day nursery (t)	0.008	0.053	0.007	0.051	0.040	0.046
childcare places available: childminder (t)	-0.062	0.084	-0.060	0.080	-0.063	0.068
childcare places available: playgroup (t)	-0.026	0.031	-0.034	0.028	-0.024	0.027
manufacturing jobs rate (t)	0.044	0.053	0.022	0.047	0.004	0.043
average KS1 class size (t+3)	0.015	0.299	-0.011	0.282	-0.023	0.234
year=2003	**6.865*****	0.351	**6.752*****	0.325	**6.791*****	0.299
year=2004	**6.542*****	0.423	**6.308*****	0.389	**6.322*****	0.364
constant	3.904	9.552	6.052	9.280	5.368	7.955
R^2 within	0.860		0.859		0.863	
R^2 between	0.262		0.219		0.203	
R^2 overall	0.336		0.288		0.280	
ρ	0.950		0.959		0.958	
#obs	345		374		419	
#groups	120		130		148	
LEA level fixed effects included	Yes		Yes		Yes	

ρ is the fraction of the variance due to the fixed effects

* p<0.10, ** p<0.05, *** p<0.01

Table A.35: The Effect of Free Early Education Places on **Maths L3+** Model #2

Dependent Variable: Percentage of children in the LEA's maintained school achieving Level 3 or higher in Key Stage 1 assessment, year (*t+3*). Data from 2006 excluded

Panel Regression Models: 3 year panel

	sample 1		sample 2		sample 3	
Independent Variable	Coeff.	Rob. St. Err.	Coeff.	Rob. St. Err.	Coeff.	Rob. St. Err.
3-year olds EE take-up, sch sector 'poorer' LEAs (t)	**-0.129***	0.070	-0.103	0.063	**-0.113*****	0.055
3-year olds EE take-up, sch sector 'better-off' LEAs (t)	0.002	0.055	0.008	0.055	0.045	0.062
3-year olds EE take-up, 'other' sector 'poorer' LEAs (t)	**-0.027***	0.015	**-0.021***	0.012	-0.017	0.012
3-year olds EE take-up, 'other' sector 'better-off' LEAs (t)	0.009	0.013	0.007	0.011	0.010	0.010
4-year olds EE take-up, sch sector 'poorer' LEAs (t+1)	0.090	0.082	0.062	0.068	0.077	0.064
4-year olds EE take-up, sch sector 'better-off' LEAs (t+1)	**0.142†**	0.086	0.122	0.083	0.048	0.083
4-year olds EE take-up, 'other' sector 'poorer' LEAs (t+1)	0.028	0.063	0.050	0.062	0.025	0.061
4-year olds EE take-up, 'other' sector 'better-off' LEAs (t+1)	0.008	0.056	0.008	0.053	-0.005	0.043
childcare places available: day nursery (t)	0.034	0.063	0.045	0.058	0.087	0.055
childcare places available: childminder (t)	0.002	0.067	0.007	0.061	0.039	0.052
childcare places available: playgroup (t)	0.012	0.029	0.012	0.026	0.018	0.026
economic inactivity rate, working age males (t)	0.025	0.036	0.038	0.035	0.038	0.035
median weekly gross pay, male f-t workers (t)	0.001	0.009	0.000	0.008	0.002	0.008
average KS1 class size (t+3)	-0.077	0.328	-0.080	0.295	0.183	0.294
% of non-white children (t+3)	-0.114	0.096	-0.115	0.094	-0.078	0.094
year=2003	**-1.718*****	0.363	**-1.851*****	0.345	**-2.062*****	0.353
year=2004	**-2.771*****	0.552	**-2.837*****	0.523	**-3.065*****	0.512
constant	**27.024*****	9.771	**27.836*****	9.314	**21.429****	8.676
R^2 within	0.544		0.550		0.520	
R^2 between	0.503		0.471		0.444	
R^2 overall	0.491		0.462		0.455	
ρ	0.902		0.898		0.816	
#obs	344		373		418	
#groups	120		130		148	
LEA level fixed effects included	Yes		Yes		Yes	

ρ is the fraction of the variance due to the fixed effects

* p<0.10, ** p<0.05, *** p<0.01, † p=0.103

A.7 Between Estimates

Clearly there is a lot of between LEA variation in the independent and the dependent variables – as witnessed in the descriptive statistics in section 6 of the paper. As outlined in the paper section 5, the problem with attempts to exploit the cross-LEA variation is that we know that the initial levels of the independent variables differ and do so in a way that is not uncorrelated with results.

To stylise the facts, poorer LEAs have higher levels of take-up of places in the maintained nursery and primary school settings, lower levels of childcare availability, and lower results. The poorer LEAs also have the free places in the 'other' sector introduced first and this leads to these LEAs having a higher average over time of provision in these settings. As a result, the between estimates will lead to negative coefficients on the take-up of 'other' sector places. Moreover, as the better off LEAs have higher results and more childcare places available, we will get positive coefficients for these variables.

We cannot conclude causal effects of these independent variables as we believe that there are unobserved characteristics of the LEAs, such as class-mix, and the attitudes and tastes of parents in the LEAs, which influence the levels of these take-up/provision variables and also affect the results variables, thus confounding attempts at causal inference.

In all of the following regressions – for each subject and level, for both the basic and augmented specification – we see these priors confirmed. For each regression we see the same pattern: a negative coefficient on 'other' sector take-up for 3-year olds, significant at the 1% level and positive coefficients on playgroup place and childminder place provision, again almost always at the 1% level of significance. In all cases there is a small negative coefficient on take-up by 3-year olds of places in the maintained nursery and primary school settings, though only in the case of writing L3 or higher is the coefficient significant and then only at the 10% level of significance.

In terms of the effect of the take-up by 3-year olds of places in the 'other' sector, the negative coefficient is driven by the unobserved characteristics of the LEAs which influence the childcare/early education provision in the LEA and also influence the results. These LEAs are poorer and as a result have lower private provision of places since, on the demand side, parents do not have a high valuation of early education and limited financial resources for this area of expenditure and therefore demand less, whereas on the supply side there are risks to private providers of setting up in these areas hence lower supply. As a result these are the LEAs that the Government targeted to receive the nursery education grant to provide 3-year olds with early education places a year earlier than in the other more prosperous LEAs. Therefore in the between estimates we get this spurious relationship that private places have a negative effect on KS1 assessment outcomes. In the second specification I can attempt to control for the observed characteristics of LEAs – such as unemployment rate, manufacturing jobs rate, median weekly gross pay – that are correlated with childcare/early education place provision/take-up and results. Even controlling for these factors I still find a significant negative coefficient on take-up in the non-maintained nursery and primary school settings. It does appear that the unobserved characteristics are biasing the coefficient.

The data descriptives in the paper reveal that there is a stark contrast between the poorer ('pathfinder') group of LEAs and the better off LEAs in terms of the use by 3-year olds of early education places in the maintained nursery and primary school settings – mean take-up in the poorer LEAs over the time period being 59.39% compared with 28.77% in the better off LEAs. However, though this is a major difference between the groups of LEAs and also correlated with the differences in results, the coefficient on this take-up rate is only significant in the writing L3 or higher regressions. The reason for this is that 3-year olds take-up of nursery and primary school places is strongly correlated with the playgroup place provision variable. 3-year olds take-up of nursery and primary school places does have a strong and significant negative coefficient in all of the regressions until the playgroup provision variable is introduced – playgroup provision has such a strong correlation with both results (as well as 3-year olds take-up of nursery and primary school places) that the coefficient on 3-year olds take-up of nursery and primary school places whilst remaining negative, reduces in size and is not longer significant.

To confirm that the (fixed) unobserved characteristics of the LEAs are correlated with the independent variables, I perform Hausman tests which determine whether the fixed effects or random effects estimation is the appropriate method to use. The table below shows for each subject and level, the Hausman test statistic for the null hypothesis that the difference in coefficients between the random effects estimation of the model and the fixed effects estimation

Subject	Level	Specification	Hausman test statistic	p-value
Reading	3 or higher	model #1	χ-squared (11) = 24.17	0.0121
		model #2	χ-squared (17) = 27.07	0.0571
	2B or higher	model #1	χ-squared (12) = 53.88	0.0000
		model #2	χ-squared (18) = 50.56	0.0001
Writing	3 or higher	model #1	χ-squared (11) = 12.29	0.3424
		model #2	χ-squared (16) = 17.85	0.3329
	2B or higher	model #1	χ-squared (12) = 30.99	0.0020
		model #2	χ-squared (18) = 37.44	0.0046
Maths	3 or higher	model #1	χ-squared (12) = 37.27	0.0000
		model #2	χ-squared (18) = -35.90	chi2<0
	2B or higher	model #1	χ-squared (13) = 63.82	0.0000
		model #2	χ-squared (19) = 9.02	0.9731

of the model is not systematic. We reject this null if the p-value on the test is less than 0.01, less than 0.05 or less than 0.10 for conventional levels of significance.

We can see that for model #1 we reject the null hypothesis at the 1% level of significance in each case except reading L3 or higher, where the rejection is at the 2% level of significance and writing L3 or higher where we fail to reject the null. In this latter case, failure to reject the null means that fixed effects estimation is still consistent though it is not as efficient as random effects estimation. For model #2 we continue to reject the null hypothesis at the 1% level for reading and writing L2B or higher, and reject the null hypothesis at just outside the 5% level for reading L3 or higher. However we continue to fail to reject the null for writing L3 or higher and strongly reject the null for maths L2B or higher. For maths L3 or higher we can no longer perform the test as the model fitted on these data fails to meet the asymptotic assumptions of the Hausman test. Again it should be borne in mind that failure to reject the null indicates that fixed effects estimation whilst still consistent may not be efficient.

It should also be borne in mind that for writing L3 or higher, the regressions implemented using fixed effects estimation and using random effects estimation produce very similar coefficients for the significant control variables (hence the failure to reject the null) however for the one significant variable of interest – take-up of places in the maintained nursery and primary school settings by 4-year olds in the better off LEAs – the coefficient is biased down to 0.078 in the random effects model from 0.157 in the fixed effect model. This suggests that the unobserved fixed characteristics of the LEAs are negatively correlated with 4-year olds take-up of places in nursery and primary school so that when we control for them in the fixed effects model the coefficient on 4-year olds take-up of places in nursery and primary school increases. This confirms the prior that there is selection into higher use of maintained settings, even within the better off LEAs group, by characteristics associated with lower outcomes.

I believe the tests, the descriptive data and the between estimates provide justification for estimating the model using fixed effects estimation.

Table A.36 to A.38 show the regression results for the between estimates of the basic specification and the specification including additional covariates, for each subject and each level.

Table A.36: The Association Between Free Early Education Place Take-Up and **Reading** Attainment

Dependent Variable: Percentage of children in the LEA's maintained school achieving Level 2B or higher in Key Stage 1 assessment, year (*t+3*).

Panel Regression Models: Between Estimates

	Level 2B or higher				**Level 3 or higher**			
	model # 1		model # 2		model # 1		model # 2	
Independent Variable	Coeff.	Rob. St. Err.	Coeff.	Rob St. Err.	Coeff.	Rob St. Err.	Coeff.	Rob St. Err.
3-year olds early educ take-up, school sector (t)	-0.033	0.034	-0.007	0.036	-0.034	0.035	-0.001	0.037
3-year olds early educ take-up, 'other' sector (t)	**-0.209*** **	0.058	**-0.162*** **	0.059	**-0.187*** **	0.048	**-0.152*** **	0.049
4-year olds early educ take-up, school sector (t+1)	0.018	0.064	0.046	0.067	0.018	0.064	0.017	0.063
4-year olds early educ olds take-up, 'other' sector (t+1)	-0.079	0.082	-0.067	0.080	0.023	0.083	0.034	0.081
childcare places available: day nursery (t)	0.135	0.109	0.127	0.106	0.099	0.103	0.112	0.100
childcare places available: childminder (t)	**0.384*** **	0.089	**0.308*** **	0.091	**0.381*** **	0.092	**0.307*** **	0.094
childcare places available: playgroup (t)	**0.186*** **	0.054	**0.179*** **	0.053	**0.155*** **	0.055	**0.139** **	0.056
economic inactivity rate, working age males (t)			**-0.267*** **	0.095			**-0.267*** **	0.098
manufacturing jobs rate (t)			-0.043	0.061				
median weekly gross pay, male f-t workers (t)							-0.006	0.006
average KS1 class size (t+3)	**-0.578* **	0.332	**-0.787** **	0.365	-0.150	0.333	-0.223	0.338
constant	**77.823*** **	11.360	**85.008*** **	11.841	**24.950** **	11.271	**32.414*** **	11.303
R^2 within	0.227		0.193		0.314		0.322	
R^2 between	0.552		0.582		0.574		0.604	
R^2 overall	0.116		0.180		0.443		0.492	
#obs	575		575		464		464	
#groups	120		120		120		120	

* p<0.10, ** p<0.05, *** p<0.01

Table A.37: The Association Between Free Early Education Place Take-Up and **Writing** Attainment

Dependent Variable: Percentage of children in the LEA's maintained school achieving Level 2B or higher in Key Stage 1 assessment, year (*t+3*).

Panel Regression Models: Between Estimates

	Level 2B or higher				**Level 3 or higher**			
	model # 1		model # 2		model # 1		model # 2	
Independent Variable	Coeff.	Rob. St. Err.	Coeff.	Rob St. Err.	Coeff.	Rob St. Err.	Coeff.	Rob St. Err.
3-year olds early educ take-up, school sector (t)	-0.056	0.040	-0.024	0.042	**-0.051***	0.030	-0.049	0.031
3-year olds early educ take-up, 'other' sector (t)	**-0.248*****	0.068	**-0.198*****	0.069	**-0.147*****	0.040	**-0.147*****	0.041
4-year olds early educ take-up, school sector (t+1)	0.021	0.074	0.035	0.078	0.059	0.054	0.054	0.057
4-year olds early educ olds take-up, 'other' sector (t+1)	-0.105	0.095	-0.091	0.093	-0.042	0.069	-0.041	0.070
childcare places available: day nursery (t)	0.158	0.126	0.150	0.124	0.028	0.086	0.028	0.086
childcare places available: childminder (t)	**0.368*****	0.103	**0.286*****	0.106	0.115	0.077	0.113	0.078
childcare places available: playgroup (t)	**0.168*****	0.063	**0.162*****	0.061	**0.079***	0.046	**0.079***	0.046
economic inactivity rate, working age males (t)			**-0.278****	0.110				
manufacturing jobs rate (t)			-0.004	0.071			0.014	0.052
average KS1 class size (t+3)	**-0.662***	0.384	**-0.772***	0.425	-0.071	0.279	-0.037	0.309
constant	**72.737*****	13.14	**77.818*****	13.788	12.100	9.445	11.353	9.900
R^2 within	0.051		0.040		0.125		0.125	
R^2 between	0.491		0.520		0.315		0.316	
R^2 overall	0.111		0.163		0.002		0.002	
#obs	575		575		464		464	
#groups	120		120		120		120	

* p<0.10, ** p<0.05, *** p<0.01

Table A.38: The Association Between Free Early Education Place Take-Up and **Maths** Attainment

Dependent Variable: Percentage of children in the LEA's maintained school achieving Level 2B or higher in Key Stage 1 assessment, year (*t+3*).

Panel Regression Models: Between Estimates

	Level 2B or higher				Level 3 or higher			
	model # 1		model # 2		model # 1		model # 2	
Independent Variable	Coeff.	Rob. St. Err.	Coeff.	Rob St. Err.	Coeff.	Rob St. Err.	Coeff.	Rob St. Err.
3-year olds early educ take-up, school sector (t)	-0.008	0.027	0.001	0.028	-0.017	0.033	0.019	0.034
3-year olds early educ take-up, 'other' sector (t)	**-0.192*****	0.047	**-0.153*****	0.046	**-0.186*****	0.045	**-0.139*****	0.046
4-year olds early educ take-up, school sector (t+1)	**-0.106****	0.052	-0.056	0.052	-0.027	0.060	-0.014	0.058
4-year olds early educ olds take-up, 'other' sector (t+1)	**-0.112***	0.065	**-0.107***	0.062	-0.003	0.077	-0.008	0.074
childcare places available: day nursery (t)	0.130	0.087	0.125	0.083	0.112	0.095	0.098	0.092
childcare places available: childminder (t)	**0.277*****	0.072	**0.220*****	0.072	**0.329*****	0.086	**0.236*****	0.088
childcare places available: playgroup (t)	**0.097****	0.044	**0.082***	0.042	0.080	0.053	**0.097***	0.052
economic inactivity rate, working age males (t)			**-0.207*****	0.073			**-0.247*****	0.089
manufacturing jobs rate (t)			**-0.126****	0.049				
median weekly gross pay, male f-t workers (t)							0.010	0.007
average KS1 class size (t+3)	0.245	0.305	-0.061	0.306	0.553	0.354	0.453	0.347
% of non-white children (t+3)	**-0.099*****	0.017	**-0.110*****	0.017	**-0.067*****	0.019	**-0.085*****	0.022
constant	**76.184*****	9.347	**85.635*****	9.345	13.390	10.838	13.340	11.296
R^2 within	0.126		0.130		0.482		0.423	
R^2 between	0.680		0.713		0.579		0.615	
R^2 overall	0.424		0.499		0.535		0.528	
#obs	574		574		463		463	
#groups	120		120		120		120	

* p<0.10, ** p<0.05, *** p<0.01

Appendix B

Appendices to Chapter 3

B.1 Estimating HCEF using only those with 11 or more years education

Table B.1: Human Capital Earnings Function Estimations, OLS and IV using Smoker at 16 Status

	OLS		IV: smoker at 16		IV: first stage	
Dep. Var: log hourly wage	Coeff.	Robust Std. Err.	Coeff.	Robust Std. Err.	Coeff.	Robust Std. Err.
constant	-0.318***	0.331	-0.845**	0.419	6.778	2.227
years of schooling	0.038***	0.003	0.132***	0.026	—	—
smoker at 16 indicator	—	—	—	—	-0.833***	0.125
age	0.106***	0.005	0.104***	0.006	0.021***	0.026
age^2	-0.001***	0.000	-0.001***	0.000	0.000***	0.000
year-of-birth	-0.026**	0.009	-0.052***	0.013	0.247***	0.059
year-of-birth^2	0.000***	0.000	0.001***	0.000	-0.002***	0.000
region: North	0.061**	0.042	0.070	0.052	-0.081	0.298
region: Yorkshire	0.017	0.038	-0.020	0.049	0.440	0.285
region: North West	0.057	0.038	0.002	0.048	0.611	0.283
region: East Midlands	-0.005	0.037	-0.011	0.045	0.107	0.268
region: East Anglia	0.008	0.044	-0.015	0.055	0.308	0.361
region: South East	0.149***	0.032	0.082**	0.043	0.762***	0.223
region: South West	0.023	0.038	0.014	0.047	0.175	0.261
region: Wales	0.008	0.046	-0.017	0.053	0.241	0.311
region: Scotland	0.039	0.040	-0.032	0.052	0.799**	0.292
ethnicity: black	0.132	0.113	0.115	0.129	0.055	0.716
ethnicity: asian	-0.165*	0.070	-0.340***	0.107	1.733***	0.484
ethnicity: other	-0.041	0.112	-0.279**	0.134	2.392*	1.103
father's occ class: 1	0.125***	0.031	0.036	0.045	0.909***	0.231
father's occ class: 2	0.144***	0.039	-0.049	0.071	1.935***	0.297
father's occ class: 3	0.082**	0.044	-0.036	0.060	1.162***	0.331
father's occ class: 4	0.085*	0.040	-0.020	0.057	1.006***	0.324
father's occ class: 5	0.038*	0.026	0.016	0.033	0.227***	0.200
father's occ class: 6	0.020	0.038	-0.072	0.054	0.873***	0.332
father's occ class: 7	0.107***	0.043	0.098	0.055	0.131	0.358
father's occ class: 9	0.002	0.037	0.058	0.046	-0.585***	0.252
father's occ class: 10	0.035	0.031	0.034	0.037	-0.047	0.223
mother's occ class: 1	0.019	0.056	0.007	0.073	0.098	0.454
mother's occ class: 2	-0.017	0.057	-0.111	0.073	1.033***	0.451
mother's occ class: 3	0.025	0.052	0.064	0.064	-0.367	0.427
mother's occ class: 4	0.024	0.043	0.005	0.055	0.238	0.345
mother's occ class: 5	-0.018	0.057	0.004	0.072	-0.097	0.489
mother's occ class: 6	0.003	0.044	0.036	0.055	-0.227	0.357
mother's occ class: 7	0.026	0.046	0.067	0.058	-0.472	0.354
mother's occ class: 9	-0.065	0.044	-0.011	0.055	-0.572	0.346
mother's occ class: 10	-0.015	0.036	-0.004	0.046	-0.105	0.305
'nuclear family' to 16	0.016	0.022	-0.009	0.026	0.180*	0.147
mid 1990s	-0.049***	0.010	-0.057***	0.011	0.086	0.051
late 1990s	-0.068***	0.016	-0.077***	0.018	0.111	0.088
post 2000	-0.033	0.023	-0.045*	0.027	0.144	0.136
# observations	16985		16985		16985	
# individuals	1739		1739		1739	
R^2	0.278		0.040		0.218	

Notes: *** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level
Standard errors are clustered at the level of the individual and robust.
'nuclear family' to 16 means lived with both natural parents from birth to age 16.
Reference categories: West Midlands, white, did not live with both natutal parents to 16, father/mother occupational class 'plant/machine operative'. Occupational Class dummies: (1) management, (2) professional, (3) associate professional/technical, (4) clerical/secretarial, (5) craft and related, (6) personal/protective services, (7) sales, (9) other, (10) self-emp/unemp.

B.2 Estimating the HCEF using only one observation per person in the first stage, Smoker at 16 IV

Table B.2: Human Capital Earnings Function Estimations, OLS and IV using Smoker at 16 Status

	OLS		IV: smoker at 16		IV: first stage	
Dep. Var: log hourly wage	**Coeff.**	**Robust Std. Err.**	**Coeff.**	**Robust Std. Err.**	**Coeff.**	**Robust Std. Err.**
constant	-0.754***	0.250	-0.541	0.351	-0.137	6.170
years of schooling	0.046***	0.003	0.133***	0.021	—	—
smoker at 16 indicator	—	—	—	—	-0.876***	0.097
age	0.099***	0.004	0.101***	0.006	-0.004	0.083
age^2	-0.001***	0.000	-0.001***	0.000	0.000	0.001
year-of-birth	-0.016***	0.007	-0.058***	0.012	0.411***	0.109
year-of-birth^2	0.000***	0.000	0.001***	0.000	-0.003***	0.001
region: North	0.047	0.038	0.061	0.044	-0.187	0.238
region: Yorkshire	0.003	0.033	0.006	0.039	0.136	0.223
region: North West	0.054	0.032	0.048	0.037	0.207	0.226
region: East Midlands	-0.010	0.032	-0.001	0.037	-0.104	0.224
region: East Anglia	0.015	0.039	0.010	0.049	0.180	0.302
region: South East	0.142***	0.028	0.102***	0.035	0.610***	0.186
region: South West	0.023	0.034	0.030	0.037	0.037	0.217
region: Wales	-0.012	0.040	-0.009	0.044	-0.042	0.270
region: Scotland	0.028	0.036	0.014	0.040	0.362	0.229
ethnicity: black	0.114	0.105	0.096	0.166	-0.034	0.881
ethnicity: asian	-0.136	0.071	-0.251***	0.092	1.385***	0.458
ethnicity: other	-0.048	0.103	-0.180	0.140	1.615*	0.841
father's occ class: 1	0.116***	0.028	0.010	0.039	1.163***	0.146
father's occ class: 2	0.121***	0.038	-0.094	0.069	2.314***	0.209
father's occ class: 3	0.089**	0.043	-0.033	0.056	1.369***	0.236
father's occ class: 4	0.065*	0.036	-0.077	0.056	1.439***	0.219
father's occ class: 5	0.038*	0.023	0.007	0.027	0.359***	0.116
father's occ class: 6	0.014	0.035	-0.073	0.046	0.933***	0.189
father's occ class: 7	0.103***	0.04	0.057	0.048	0.490**	0.219
father's occ class: 9	-0.021	0.029	0.025	0.037	-0.494***	0.132
father's occ class: 10	0.029	0.027	0.007	0.030	0.152	0.126
mother's occ class: 1	0.047	0.049	0.025	0.064	0.159	0.296
mother's occ class: 2	0.015	0.054	-0.108	0.071	1.379***	0.313
mother's occ class: 3	0.056	0.048	0.032	0.063	0.179	0.312
mother's occ class: 4	0.055	0.04	0.008	0.049	0.491***	0.225
mother's occ class: 5	0.01	0.049	0.019	0.062	-0.044	0.263
mother's occ class: 6	0.025	0.04	0.030	0.049	0.031	0.228
mother's occ class: 7	0.055	0.041	0.054	0.049	-0.082	0.227
mother's occ class: 9	-0.004	0.038	0.030	0.049	-0.439***	0.201
mother's occ class: 10	0.004	0.032	-0.010	0.039	0.138	0.176
'nuclear family' to 16	0.028	0.019	-0.006	0.023	0.290	0.097
mid 1990s	-0.045***	0.009	-0.039	0.015	-0.130	0.211
late 1990s	-0.065***	0.014	-0.056	0.023	-0.144	0.347
post 2000	-0.033	0.021	-0.023	0.034	-0.055	0.533
# observations	21256		13498		1432	
# individuals	2266		1398		1432	
R^2	0.265		0.220		0.250	

F-test on exclusion of instrument from first stage: 51.50; Partial R^2 of the instrument = 0.0302

Notes: *** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level
'nuclear family' to 16 means lived with both natural parents from birth to age 16.
Reference categories: West Midlands, white, did not live with both natutal parents to 16, father/mother occupational class 'plant/machine operative'. Occupational Class dummies: (1) management, (2) professional, (3) associate professional/technical, (4) clerical/secretarial, (5) craft and related, (6) personal/protective services, (7) sales, (9) other, (10) self-emp/unemp.
IV second stage standard errors obtained by bootstrapping.

B.3 Estimating the HCEF using only one observation per person in the first stage, RoSLA IV

Table B.3: Human Capital Earnings Function Estimations, OLS and IV using RoSLA

	OLS		IV: RoSLA		IV: first stage	
Dep. Var: log hourly wage	**Coeff.**	**Robust Std. Err.**	**Coeff.**	**Robust Std. Err.**	**Coeff.**	**Robust Std. Err.**
constant	-0.754***	0.250	-0.533	0.377	-0.564	5.011
years of schooling	0.046***	0.003	0.143**	0.058		
smoker at 16 indicator	—	—	—	—	0.487***	0.153
age	0.099***	0.004	0.101***	0.006	0.020	0.070
age^2	-0.001***	0.000	-0.001***	0.000	0.000	0.001
year-of-birth	-0.016***	0.007	-0.062**	0.028	0.408***	0.091
year-of-birth^2	0.000***	0.000	0.001***	0.000	-0.004***	0.001
region: North	0.047	0.038	0.058	0.048	-0.132	0.192
region: Yorkshire	0.003	0.033	0.000	0.045	0.142	0.178
region: North West	0.054	0.032	0.047	0.041	0.176	0.175
region: East Midlands	-0.010	0.032	0.004	0.039	-0.139	0.174
region: East Anglia	0.015	0.039	0.007	0.054	0.121	0.243
region: South East	0.142***	0.028	0.093*	0.049	0.609***	0.141
region: South West	0.023	0.034	0.026	0.039	-0.005	0.171
region: Wales	-0.012	0.040	-0.006	0.047	-0.023	0.211
region: Scotland	0.028	0.036	0.007	0.048	0.388**	0.185
ethnicity: black	0.114	0.105	0.102	0.181	0.123	0.788
ethnicity: asian	-0.136	0.071	-0.270**	0.134	1.571***	0.377
ethnicity: other	-0.048	0.103	-0.202	0.173	1.756**	0.695
father's occ class: 1	0.116***	0.028	-0.004	0.078	1.217***	0.149
father's occ class: 2	0.121***	0.038	-0.120	0.155	2.443***	0.213
father's occ class: 3	0.089**	0.043	-0.052	0.101	1.474***	0.244
father's occ class: 4	0.065*	0.036	-0.093	0.102	1.566***	0.222
father's occ class: 5	0.038*	0.023	0.001	0.035	0.359***	0.118
father's occ class: 6	0.014	0.035	-0.084	0.074	0.994***	0.196
father's occ class: 7	0.103***	0.040	0.050	0.057	0.535**	0.223
father's occ class: 9	-0.021	0.029	0.029	0.048	-0.522***	0.133
father's occ class: 10	0.029	0.027	0.003	0.034	0.210	0.128
mother's occ class: 1	0.047	0.049	0.025	0.073	0.132	0.303
mother's occ class: 2	0.015	0.054	-0.130	0.108	1.373***	0.321
mother's occ class: 3	0.056	0.048	0.030	0.068	0.150	0.317
mother's occ class: 4	0.055	0.040	-0.001	0.058	0.485**	0.234
mother's occ class: 5	0.010	0.049	0.014	0.069	-0.112	0.274
mother's occ class: 6	0.025	0.040	0.027	0.052	-0.056	0.234
mother's occ class: 7	0.055	0.041	0.059	0.054	-0.080	0.233
mother's occ class: 9	-0.004	0.038	0.037	0.061	-0.471**	0.208
mother's occ class: 10	0.004	0.032	-0.013	0.042	0.135	0.184
'nuclear family' to 16	0.028	0.019	-0.011	0.033	0.376***	0.097
mid 1990s	-0.045***	0.009	-0.039**	0.015	-0.161	0.209
late 1990s	-0.065***	0.014	-0.056**	0.023	-0.137	0.349
post 2000	-0.033	0.021	-0.024	0.034	-0.030	0.539
# observations	21256		13498		1398	
# individuals	2266		1398		1398	
R^2	0.265		0.160		0.229	

F-test on exclusion of instrument from first stage: 4.06; Partial R^2 of the instrument = 0.0029

Notes: *** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level
'nuclear family' to 16 means lived with both natural parents from birth to age 16.
Reference categories: West Midlands, white, did not live with both natutal parents to 16, father/mother occupational class 'plant/machine operative'. Occupational Class dummies: (1) management, (2) professional, (3) associate professional/technical, (4) clerical/secretarial, (5) craft and related, (6) personal/protective services, (7) sales, (9) other, (10) self-emp/unemp.
IV second stage standard errors obtained by bootstrapping.

Appendix C

Appendices to Chapter 4

C.1 Education Breakdown by Country

Education Level	Germany %	Neth. %	France %	Italy %	Spain %	Portugal %
"high"	27.2	24.8	25.8	9.0	33.6	7.8
"medium"	61.4	55.1	45.7	51.6	23.0	15.6
"low"	11.3	20.1	28.5	39.4	43.4	76.6
Total	100.0	100.0	100.0	100.0	100.0	100.0

Table C.1: Education Breakdown by Country

"High" education refers to ISCED levels 5-7, corresponding to any tertiary level education. "Medium" level education refers to ISCED level 3 and corresponds to upper-secondary (i.e. post-compulsory) level schooling, while "low" education refers to ISCED levels 0-2 and represents levels of education up to the end of compulsory schooling. The Table shows that for Germany, the Netherlands, France and Spain the proportion of "high" educated individuals is of a similar order of magnitude, however Italy and Portugal have a considerably smaller proportion of individuals in the top educated bracket. As the ECHP surveys are designed to be the same in each country and the education coding is a standard international classification, this should be reflecting genuine differences in educational composition of each sample. Ideally the proportion with "high" education would approximately similar in each country, which is not the case, primarily due to Italy and Portugal. An alternative strategy would be to capture human capital differences via occupational classification. The ECHP contains the International Standard Classification of Occupation (ISCO-88) 1-digit level classification for individual's occupations. The 1-digit ISCO-88 classification assigns occupations to one of 9 categories, from 1 "Legislators, senior officials, managers", through 5 "Service workers and shop and market sales workers", to 9 "Elementary occupations". Attempts to combine these gradings into 3 broad levels of human capital attainment, with similar proportions of individuals at each level in each country, were unsuccessful. Consequently, though dividing individuals using the 3 category education variable does not result in absolute symmetry across countries, it is more satisfactory than the possible alternative human capital measures based on occupational classification.

C.2 Model Specification

In this appendix we describe in full detail the functional form assumptions of the model. To refresh the notation and basic structure of the statistical model: each country's sample is a set of N workers indexed i =1, ..., N, each of whom is followed over T_i consecutive years. A typical individual observation i is a vector $\mathbf{x}_i = \left(\mathbf{y}_i, \mathbf{e}_i, \mathbf{pub}_i, \mathbf{z}_i^v, z_i^f\right)$, to which we append a pair of unobserved class indexes, $k_i = (k_i^m, k_i^y)$. As outlined in Section 4.4 of the main body, there are three components to individual i's contribution to the complete likelihood (equation (4.1), referring respectively to unobserved heterogeneity, labour market status history and earnings history. Below we set out the full specification of each of these components. The choice of covariates to be included in each component was informed not only by the descriptive analysis of section 4.3, but also by a concern for numerical tractability, parsimony and the aim to have the same model specification estimated for each of the countries.

C.2.1 Unobserved Heterogeneity

As outlined in equation (4.2), the attachment of individual i to a given latent class $k_i = (k_i^m, k_i^y)$ is modelled as the product of two terms: $\ell_i\left(k_i \mid z_i^f\right) = \Pr\left\{k_i^y \mid k_i^m, z_i^f\right\} \cdot \Pr\left\{k_i^m \mid z_i^f\right\}$, which are both specified as multinomial logits:

$$\Pr\left\{k_i^m = k^m \mid z_i^f\right\} = \frac{\exp\left(z_i^{f\prime} \cdot \kappa_{k^m}^m\right)}{\sum_{k=1}^{K^m} \exp\left(z_i^{f\prime} \cdot \kappa_k^m\right)} \quad \text{and} \quad \Pr\left\{k_i^y \mid k_i^m, z_i^f\right\} = \frac{\exp\left[\begin{pmatrix} z_i^f \\ k_i^m \end{pmatrix}^{\prime} \cdot \kappa_{k^y}^y\right]}{\sum_{k=1}^{K^y} \exp\left[\begin{pmatrix} z_i^f \\ k_i^m \end{pmatrix}^{\prime} \cdot \kappa_k^y\right]}, \tag{C.21}$$

where κ_1^m and κ_1^y are both normalised at zero.

C.2.2 Labour Market States

Equations (4.3), (4.4) and (4.5) established that the individual labour market histories contribute to the complete likelihood as:

$$\ell_i\left(\mathbf{S}_i \mid \mathbf{z}_i^v, z_i^f, k_i^m\right) = \Pr\left\{S_{i1} \mid z_i^f, k_i^m\right\} \times \prod_{t=2}^{T_i} \Pr\left\{S_{it} \mid S_{i,t-1}, z_{i,t-1}^v, z_i^f, k_i^m\right\}, \tag{C.22}$$

We can express this rather in terms of the indicator variables e_{it} and pub_{it} as:

$$\begin{aligned} \ell_i\left(\mathbf{e}_i, \mathbf{pub}_i \mid \mathbf{z}_i^v, z_i^f, k_i^m\right) &= \Pr\left\{e_{i1} \mid z_i^f, k_i^m\right\} \times \left[\Pr\left\{\text{pub}_{i1} \mid z_i^f, k_i^m\right\}\right]^{e_{it}} \\ &\times \prod_{t=2}^{T_i} \left(\Pr\left\{e_{it} \mid e_{i,t-1}, \text{pub}_{i,t-1}, z_{i,t-1}^v, z_i^f, k_i^m\right\} \times \left[\Pr\left\{\text{pub}_{it} \mid e_{i,t-1}, \text{pub}_{i,t-1}, z_{i,t-1}^v, z_i^f, k_i^m\right\}\right]^{e_{it}}\right). \end{aligned} \tag{C.23}$$

As alluded to in subsection (4.4.3), each component is specified as a logit. Allowing $\Lambda(x) = (1+e^{-x})^{-1}$ to designate the logistic cdf:

$$\Pr\left\{e_{it} \mid e_{i,t-1}, \text{pub}_{i,t-1}, z^v_{i,t-1}, z^f_i, k^m_i\right\} = \Lambda\left(\left[e_{i,t-1}, \text{pub}_{i,t-1}, z^v_{i,t-1}{}', z^f_i{}', k^m_i{}'\right] \cdot \psi\right),$$
$$\Pr\left\{\text{pub}_{it} \mid e_{i,t-1}, \text{pub}_{i,t-1}, z^v_{i,t-1}, z^f_i, k^m_i\right\} = \Lambda\left(\left[e_{i,t-1}, \text{pub}_{i,t-1}, z^v_{i,t-1}{}', z^f_i{}', k^m_i{}'\right] \cdot \chi\right), \quad \text{(C.24)}$$

We allow the unobserved mobility heterogeneity class k^m_i to affect the unemployment and public sector probabilities only through altering the constant terms in the respective logits. This is because the number of observed sector transitions is not sufficient in the sample for most of the countries to allow less restrictive specifications – such as allowing this unobserved class to interact with experience or education – to be estimated. We do however allow the effect of experience (and it's square) to interact with previous state. For the initial job state probabilities, we use similar specifications[1]:

$$\Pr\left\{e_{i1} \mid z^f_i, k^m_i\right\} = \Lambda\left(\left[z^f_i{}', k^m_i{}'\right] \cdot \psi_0\right) \text{ and } \Pr\left\{\text{pub}_{i1} \mid z^f_i, k^m_i\right\} = \Lambda\left(\left[z^f_i{}', k^m_i{}'\right] \cdot \chi_0\right) \quad \text{(C.25)}$$

C.2.3 Earnings

As the exposition in the main body text Section (4.4.4) details much of the modelling of earnings trajectories, what remains for this appendix is to set out the set of functions $\{\mu(\cdot), \sigma(\cdot), \tau_1(\cdot) \text{ and } \tau_2(\cdot)\}$ introduced in equations (4.7) and (4.9). Recall from Section (4.4.4) that only individuals who are employed at date-t have earnings information available at date-t, therefore $e_{it} = 1$ for all observations used to estimate the $\mu(\cdot)$, and indeed the $\sigma(\cdot)$ function, and as such e_{it} is not an argument of either function. Starting with $\mu(\cdot)$, we posit that:

$$\mu\left(\text{pub}_{it}, e_{i,t-1}, z^v_{it}, z^f_i, k^y_i\right) = \begin{bmatrix} z^f_i \\ e_{i,t-1} \end{bmatrix}' \mu_0 + [z^v_{it} * \text{pub}_{it}]' \mu_1 + [k^y_i * z^v_{it}]' \mu_2 + [k^y_i \cdot \text{pub}_{it}] \mu_3, \quad \text{(C.26)}$$

where the notation $x * y$ stands for all of the main effects and interactions of variables x and y, and $x \cdot y$ stands for the single interaction term between x and y. Thus the specification of the $\mu(\cdot)$ function allows the effect of experience to differ across job sectors and wage classes, and the public sector effect is also allowed to vary with wage class. Previous period unemployment and time-invariant heterogeneity can affect the intercept only.

[1] Again the unobserved mobility heterogeneity class k^m_i can only alter the constant term in each equation, and for the initial states we do not allow interactions of experience with previous state

Turning to the log earnings variance function, we specify:

$$\sigma\left(\text{pub}_{it}, e_{i,t-1}, z_{it}^{v}, k_{i}^{y}\right) = \sqrt{\exp\left(\begin{bmatrix} z_{it}^{v} \\ \text{pub}_{it} \\ e_{i,t-1} \\ k_{i}^{y} \end{bmatrix}' \cdot \sigma_0\right)}. \tag{C.27}$$

Clearly the functional form posited for $\sigma(\cdot)$ is considerably more restrictive than we allow for the earnings means. Specifically we do not include the time-invariant observed individual characteristics z_i^f amongst the arguments of $\sigma(\cdot)$, thus we allow them to influence earnings variance only through their link to the time-invariant wage class, k_i^y. Moreover, we do not allow interactions of the wage class with any of the other arguments. Given the relatively small sample sizes available, and some experimentation with allowing some interactions, between for example k_i^y and pub_{it}, we find this specification to provide the best fit for all of the countries in the data. Note that by specifying it as an exponential, we force the log earnings variance to be positive.

Finally, we come to the specification of the earnings dynamics, which are governed by the functions $\tau_1(\cdot)$ and $\tau_2(\cdot)$. Again recall that earnings at date-t are only available for individuals in employment at that date and therefore $e_{it} = 1$ and $e_{i,t-1} = 1$ for all observations contributing to the estimation of the $\tau_1(\cdot)$ function and as such are not arguments of the function. The first-order auto-correlation of earnings, $\tau_1(\cdot)$, is posited as:

$$\begin{aligned} &\tau_1\left(\text{pub}_{it}, \text{pub}_{i,t-1}, z_{it}^{v}, z_{i}^{f}, k_{i}^{y}\right) \\ &\qquad = -1 + 2\cdot\Lambda\left\{\left[z_{it}^{f} * k_{i}^{y}\right]' \cdot \zeta_0 + \left[\begin{pmatrix} z_{it}^{v} \\ \text{pub}_{it} \end{pmatrix} * k_{i}^{y}\right]' \cdot \zeta_1 + \left[\text{pub}_{i,t-1} * k_{i}^{y}\right]' \cdot \zeta_2\right\}. \end{aligned} \tag{C.28}$$

This specification requires some clarification. Firstly, the transformation $-1 + 2\cdot\Lambda(\cdot)$ which we apply to a linear index in the explanatory variables is there to constrain $\tau_1(\cdot)$, which is a correlation coefficient, to lie within $[-1, +1]$. Second, as with the specification of $\sigma(\cdot)$ function, the number of interactions amongst the covariates is limited to allowing different impacts of each covariate depending on the wage class. This specification was decided upon following numerous trials involving different specifications with various interactions permitted. The finding was that the vast increase in computation time that this entailed for each country, did not bring any clear benefit in terms of greater precision of the fit, thus the current more parsimonious specification was settled upon.

The correlation between normalised log earnings and normalised log earnings lagged twice, $\tau_2(\cdot)$, is more complex. First let us recall the notation introduced in Section 4.4.4's equation (4.9), for the one-and two-lag autocorrelations of earnings at date-t:

$$\begin{aligned} \tau_{i,t,t-1} &= \tau_1\left(\mathrm{pub}_{it}, \mathrm{pub}_{i,t-1}, z_{it}^v, z_i^f, k_i^y\right) \\ \text{and} \quad \tau_{i,t,t-2} &= \tau_2\left(\mathrm{pub}_{it}, \mathrm{pub}_{i,t-1}, \mathrm{pub}_{i,t-2}, z_{it}^v, z_i^f, k_i^y\right). \end{aligned}$$

Now we write:

$$\begin{aligned} \tau_2\left(\mathrm{pub}_{it}, \mathrm{pub}_{i,t-1}, \mathrm{pub}_{i,t-2}, z_{it}^v, z_i^f, k_i^y\right) &= \tau_{i,t,t-1} \cdot \tau_{i,t-1,t-2} \\ &+ \left[\sqrt{\left(1-\tau_{i,t,t-1}^2\right) \cdot \left(1-\tau_{i,t-1,t-2}^2\right)}\right] \cdot \widetilde{\tau}_2\left(k_i^y\right), \end{aligned} \tag{C.29}$$

with $\widetilde{\tau}_2\left(k_i^y\right) = -1 + 2 \cdot \Lambda\left(k_i^{y\prime} \cdot \xi\right)$ simply specified as an wage class-specific constant within $[-1,+1]$. Note that $\tau_{i,t-1,t-2}$ is simply the first lag of $\tau_{i,t,t-1}$.

These latter equations require some comments. Firstly, we have to constrain $\tau_2(\cdot)$ in such a way that, given $\tau_{i,t,t-1}$ and $\tau_{i,t-1,t-2}$, the matrix:

$$\underline{\tau}_{it}^{(3)} = \begin{pmatrix} 1 & \tau_{i,t,t-1} & \tau_{i,t,t-2} \\ \tau_{i,t,t-1} & 1 & \tau_{i,t-1,t-2} \\ \tau_{i,t,t-2} & \tau_{i,t-1,t-2} & 1 \end{pmatrix}$$

is a consistent covariance matrix. This is the case provided that its determinant Δ_{it} is positive (and that the various τ's lie in $[-1,+1]$). Δ_{it} is defined by $\Delta_{it} = 1 - \tau_{i,t,t-1}^2 - \tau_{i,t-1,t-2}^2 - \tau_{i,t,t-2}^2 + 2\tau_{i,t,t-1}\tau_{i,t-1,t-2}\tau_{i,t,t-2}$. Solving for $\tau_{i,t,t-2}$, we get:

$$\tau_{i,t,t-2} = \tau_{i,t,t-1} \cdot \tau_{i,t-1,t-2} \pm \sqrt{\left(1-\tau_{i,t,t-1}^2\right) \cdot \left(1-\tau_{i,t-1,t-2}^2\right) - \Delta_{it}}. \tag{C.210}$$

Because Δ_{it} is positive, $\tau_{i,t,t-2}$ has to stay within the interval

$$\left[\tau_{i,t,t-1} \cdot \tau_{i,t-1,t-2} - \sqrt{\left(1-\tau_{i,t,t-1}^2\right) \cdot \left(1-\tau_{i,t-1,t-2}^2\right)}, \tau_{it}^1 \cdot \tau_{i,t-1}^1 + \sqrt{\left(1-\tau_{i,t,t-1}^2\right) \cdot \left(1-\tau_{i,t-1,t-2}^2\right)}\right].$$

This is achieved by the parameterization in equation (C.29) given the constraint $\widetilde{\tau}_2(\cdot) \in [-1,+1]$.

C.3 Parameter Estimates

C.3.1 Germany

Initial unemployment probability: $\Pr\left\{e_{i1}=0 \mid z_i^f, k_i^m\right\}$			
Experience (years/10)	0.121 (0.280)	Experience^2 (years^2/100)	0.000 (0.066)
High education	−0.818 (0.235)	Medium education	−0.779 (0.244)
$k^m = 2$	1.495 (1.077)	$k^m = 3$	0.443 (1.080)
Constant	−0.922 (1.090)		

Initial probability of public sector: $\Pr\left\{\mathrm{pub}_{i1}=1 \mid e_{i1}=1, z_i^f, k_i^m\right\}$			
Experience (years/10)	−0.159 (0.411)	Experience^2 (years^2/100)	1.272 (0.416)
High education	−0.224 (0.097)	Medium education	1.610 (0.308)
$k^m = 2$	−5.991 (0.327)	$k^m = 3$	−3.975 (0.458)
Constant	−0.483 (0.434)		

Table C.2: Germany: Parameters of job sector mobility (logit models), initial conditions

Unemployment prob.: $\Pr\left\{e_{it}=0 \mid e_{i,t-1}, \text{pub}_{i,t-1}, z^v_{i,t-1}, z^f_i, k^m_i\right\},\ t \geq 2$			
Experience (years/10)	−1.359 (0.167)	Experience^2 (years^2/100)	0.270 (0.035)
High Education	−0.420 (0.130)	Medium Education	−0.018 (0.101)
Public last period: $\text{pub}_{i,t-1}=1$	0.418 (0.555)	Public last period × Experience	0.100 (0.536)
Public last period × Experience^2	−0.010 (0.113)	Unempl. last period: $e_{i,t-1}=0$	1.158 (0.296)
Unempl. last period × Experience	0.344 (0.298)	Unempl. last period × Experience^2	0.030 (0.064)
$k^m=2$	−3.093 (0.107)	$k^m=3$	−5.250 (0.379)
Constant	−0.017 (0.195)		

Prob. of public sector: $\Pr\left\{\text{pub}_{it}=0 \mid e_{it}=1, e_{i,t-1}, \text{pub}_{i,t-1}, z^v_{i,t-1}, z^f_i, k^m_i\right\},\ t \geq 2$			
Experience (years/10)	−0.529 (0.280)	Experience^2 (years^2/100)	0.094 (0.062)
High Education	0.467 (0.216)	Medium Education	0.350 (0.204)
Public last period: $\text{pub}_{i,t-1}=1$	3.639 (0.360)	Public last period × Experience	1.236 (0.377)
Public last period × Experience^2	−0.189 (0.084)	Unempl. last period: $e_{i,t-1}=0$	1.336 (0.512)
Unempl. last period × Experience	0.722 (0.548)	Unempl. last period × Experience^2	−0.122 (0.126)
$k^m=2$	−1.422 (0.169)	$k^m=3$	1.690 (0.141)
Constant	−3.701 (0.347)		

Table C.3: Germany: Parameters of job sector mobility (logit models), subsequent years

Earnings means: $\mu\left(\text{pub}_{it}, e_{i,t-1}, z^v_{it}, z^f_i, k^y_i\right)$			
High education	0.342 (0.005)	Medium education	0.036 (0.004)
Experience (years/10)	0.041 (0.007)	Experience^2 (years^2/100)	-0.007 (0.001)
Public: $\text{pub}_{it} = 1$	-0.195 (0.011)	Experience×Public	0.148 (0.011)
Experience^2×Public	-0.022 (0.002)	Unemployed$_{i,t-1}$	-0.256 (0.007)
$k^y = 2$	-0.066 (0.010)	$(k^y = 2)$ ×Experience	0.053 (0.010)
$(k^y = 2)$ ×Experience^2	-0.015 (0.002)	$(k^y = 2)$ ×Public	0.030 (0.006)
Constant	8.321 (0.008)		
Earnings standard deviations: $\sigma\left(\text{pub}_{it}, e_{i,t-1}, z^v_{it}, k^y_i\right)$			
Experience (years/10)	0.038 (0.015)	Public: $\text{pub}_{it} = 1$	-0.157 (0.041)
$k^y = 2$	0.495 (0.033)	Unemployed$_{i,t-1}$	0.067 (0.087)
Constant	-3.932 (0.041)		

Table C.4: Germany: Parameters of cross-sectional earnings means and standard deviations

First-order earnings autocorrelation: $\tau_1\left(\text{pub}_{it}, \text{pub}_{i,t-1}, z^v_{it}, k^y_i\right)$			
$(k^y = 1)$ ×(High Education)	1.136 (0.055)	$(k^y = 1)$ ×(Medium Education)	-0.592 (0.049)
$(k^y = 1)$ ×Public	0.017 (0.079)	$(k^y = 1)$ ×(Public last period)	-0.326 (0.078)
$(k^y = 1)$ ×(Experience (years/10))	-0.268 (0.014)	$k^y = 1$	-2.526 (0.057)
$(k^y = 2)$ ×(High Education)	-3.063 (0.107)	$(k^y = 2)$ ×(Medium Education)	-0.721 (0.104)
$(k^y = 2)$ ×Public	-0.020 (0.089)	$(k^y = 2)$ ×(Public last period)	-0.428 (0.088)
$(k^y = 2)$ ×(Experience (years/10))	-0.258 (0.018)	$k^y = 2$	-0.975 (0.114)
Second-order earnings autocorrelation: $\widetilde{\tau}_2\left(k^y_i\right)$			
$k^y = 1$	-0.661 (0.025)	$k^y = 2$	-0.568 (0.032)

Table C.5: Germany: Parameters of earnings mobility

Mobility heterogeneity: $\Pr\left\{k_i^m = 2 \mid z_i^f\right\}$			
Experience (years/10)	-0.273 (0.041)	High Education	0.771 (0.161)
Medium education	0.133 (0.131)	Constant	1.134 (0.150)

Mobility heterogeneity: $\Pr\left\{k_i^m = 3 \mid z_i^f\right\}$			
Experience (years/10)	-0.117 (0.050)	High Education	2.351 (0.253)
Medium education	1.389 (0.234)	Constant	-1.309 (0.250)

Earnings heterogeneity: $\Pr\left\{k_i^y = 2 \mid k_i^m, z_i^f\right\}$			
Experience (years/10)	-0.063 (0.035)	High Education	1.361 (0.151)
Medium education	1.114 (0.138)	$k^m = 2$	-1.422 (0.103)
$k^m = 3$	-1.556 (0.121)	Constant	0.085 (0.159)

Table C.6: Germany: Parameters of unobserved heterogeneity (multinomial logit models)

C.3.2 Netherlands

Initial unemployment probability: $\Pr\left\{e_{i1} = 0 \mid z_i^f, k_i^m\right\}$			
Experience (years/10)	-3.186 (0.429)	Experience^2 (years^2/100)	0.719 (0.116)
High education	-0.942 (0.390)	Medium education	-0.829 (0.319)
$k^m = 2$	2.098 (0.687)	$k^m = 3$	0.553 (0.690)
Constant	-2.023 (0.704)		

Initial probability of public sector: $\Pr\left\{\text{pub}_{i1} = 1 \mid e_{i1} = 1, z_i^f, k_i^m\right\}$			
Experience (years/10)	0.780 (0.424)	Experience^2 (years^2/100)	-0.199 (0.113)
High education	1.171 (0.372)	Medium education	1.062 (0.361)
$k^m = 2$	-18.274 (304.200)	$k^m = 3$	-35.843 (697.896)
Constant	16.018 (304.200)		

Table C.7: Netherlands: Parameters of job sector mobility (logit models), initial conditions

Unemployment prob.: $\Pr\left\{e_{it}=0 \mid e_{i,t-1}, \text{pub}_{i,t-1}, z^v_{i,t-1}, z^f_i, k^m_i\right\}$, $t \geq 2$			
Experience (years/10)	−1.410 (0.298)	Experience^2 (years^2/100)	0.365 (0.064)
High Education	−0.785 (0.216)	Medium Education	−0.237 (0.161)
Public last period: $\text{pub}_{i,t-1}=1$	1.640 (0.591)	Public last period × Experience	−2.245 (0.641)
Public last period × Experience^2	0.535 (0.140)	Unempl. last period: $e_{i,t-1}=0$	2.353 (0.428)
Unempl. last period × Experience	0.174 (0.501)	Unempl. last period × Experience^2	0.104 (0.114)
$k^m=2$	2.962 (0.395)	$k^m=3$	0.886 (0.419)
Constant	−4.633 (0.491)		

Prob. of public sector: $\Pr\left\{\text{pub}_{it}=0 \mid e_{it}=1, e_{i,t-1}, \text{pub}_{i,t-1}, z^v_{i,t-1}, z^f_i, k^m_i\right\}$, $t \geq 2$			
Experience (years/10)	0.576 (0.356)	Experience^2 (years^2/100)	−0.113 (0.080)
High Education	0.749 (0.173)	Medium Education	0.582 (0.168)
Public last period: $\text{pub}_{i,t-1}=1$	3.144 (0.497)	Public last period × Experience	0.159 (0.484)
Public last period × Experience^2	−0.082 (0.107)	Unempl. last period: $e_{i,t-1}=0$	1.993 (0.737)
Unempl. last period × Experience	−1.593 (0.952)	Unempl. last period × Experience^2	0.383 (0.235)
$k^m=2$	−2.723 (0.145)	$k^m=3$	−7.696 (0.435)
Constant	−0.631 (0.391)		

Table C.8: Netherlands: Parameters of job sector mobility (logit models), subsequent years

Earnings means: $\mu\left(\text{pub}_{it}, e_{i,t-1}, z_{it}^{v}, z_{i}^{f}, k_{i}^{y}\right)$			
High education	0.454 (0.004)	Medium education	0.114 (0.003)
Experience (years/10)	0.324 (0.007)	Experience^2 (years^2/100)	−0.053 (0.002)
Public: $\text{pub}_{it} = 1$	−0.122 (0.012)	Experience×Public	0.002 (0.011)
Experience^2×Public	0.005 (0.002)	Unemployed$_{i,t-1}$	−0.141 (0.010)
$k^y = 2$	0.005 (0.009)	$(k^y = 2)$ ×Experience	−0.013 (0.009)
$(k^y = 2)$ ×Experience^2	−0.002 (0.002)	$(k^y = 2)$ ×Public	0.136 (0.005)
Constant	8.168 (0.007)		
Earnings standard deviations: $\sigma\left(\text{pub}_{it}, e_{i,t-1}, z_{it}^{v}, k_{i}^{y}\right)$			
Experience (years/10)	0.063 (0.019)	Public: $\text{pub}_{it} = 1$	−0.375 (0.046)
$k^y = 2$	−0.114 (0.039)	Unemployed$_{i,t-1}$	−0.082 (0.183)
Constant	−4.016 (0.050)		

Table C.9: Netherlands: Parameters of cross-sectional earnings means and standard deviations

First-order earnings autocorrelation: $\tau_1\left(\text{pub}_{it}, \text{pub}_{i,t-1}, z_{it}^{v}, k_{i}^{y}\right)$			
$(k^y = 1)$ ×(High Education)	1.113 (0.055)	$(k^y = 1)$ ×(Medium Education)	1.150 (0.048)
$(k^y = 1)$ ×Public	−0.829 (0.083)	$(k^y = 1)$ ×(Public last period)	−0.652 (0.082)
$(k^y = 1)$ ×(Experience (years/10))	−0.203 (0.020)	$k^y = 1$	−3.190 (0.055)
$(k^y = 2)$ ×(High Education)	−2.171 (0.085)	$(k^y = 2)$ ×(Medium Education)	−2.286 (0.077)
$(k^y = 2)$ ×Public	0.933 (0.103)	$(k^y = 2)$ ×(Public last period)	0.950 (0.101)
$(k^y = 2)$ ×(Experience (years/10))	−0.321 (0.020)	$k^y = 2$	−1.143 (0.086)
Second-order earnings autocorrelation: $\widetilde{\tau}_2\left(k_{i}^{y}\right)$			
$k^y = 1$	−0.535 (0.033)	$k^y = 2$	−0.576 (0.033)

Table C.10: Netherlands: Parameters of earnings mobility

Mobility heterogeneity: $\Pr\left\{k_i^m = 2 \mid z_i^f\right\}$			
Experience (years/10)	-0.653 (0.073)	High Education	-0.916 (0.213)
Medium education	-0.415 (0.207)	Constant	1.726 (0.223)

Mobility heterogeneity: $\Pr\left\{k_i^m = 3 \mid z_i^f\right\}$			
Experience (years/10)	-0.439 (0.059)	High Education	-1.461 (0.182)
Medium education	-0.050 (0.172)	Constant	2.571 (0.197)

Earnings heterogeneity: $\Pr\left\{k_i^y = 2 \mid k_i^m, z_i^f\right\}$			
Experience (years/10)	0.012 (0.045)	High Education	1.303 (0.146)
Medium education	1.512 (0.128)	$k^m = 2$	1.513 (0.162)
$k^m = 3$	1.469 (0.136)	Constant	-2.499 (0.192)

Table C.11: Netherlands: Parameters of unobserved heterogeneity (multinomial logit models)

C.3.3 France

Initial unemployment probability: $\Pr\left\{e_{i1} = 0 \mid z_i^f, k_i^m\right\}$			
Experience (years/10)	-1.307 (0.313)	Experience^2 (years^2/100)	0.520 (0.091)
High education	0.103 (0.268)	Medium education	0.334 (0.239)
$k^m = 2$	-6.867 (0.764)	$k^m = 3$	-4.206 (0.323)
Constant	0.030 (0.255)		

Initial probability of public sector: $\Pr\left\{\text{pub}_{i1} = 1 \mid e_{i1} = 1, z_i^f, k_i^m\right\}$			
Experience (years/10)	0.700 (0.254)	Experience^2 (years^2/100)	-0.268 (0.062)
High education	1.799 (0.197)	Medium education	0.469 (0.153)
$k^m = 2$	1.894 (0.239)	$k^m = 3$	-2.173 (0.235)
Constant	-1.967 (0.257)		

Table C.12: France: Parameters of job sector mobility (logit models), initial conditions

Unemployment prob.: $\Pr\left\{e_{it}=0 \mid e_{i,t-1}, \mathrm{pub}_{i,t-1}, z^v_{i,t-1}, z^f_i, k^m_i\right\},\ t \geq 2$			
Experience (years/10)	-0.831 (0.265)	Experience^2 (years^2/100)	0.406 (0.059)
High Education	-0.373 (0.152)	Medium Education	-0.051 (0.125)
Public last period: $\mathrm{pub}_{i,t-1}=1$	1.098 (0.598)	Public last period $\times$ Experience	-3.532 (0.987)
Public last period $\times$ Experience^2	1.144 (0.278)	Unempl. last period: $e_{i,t-1}=0$	1.970 (0.319)
Unempl. last period $\times$ Experience	0.022 (0.410)	Unempl. last period $\times$ Experience^2	0.129 (0.103)
$k^m=2$	-29.375 (5725.096)	$k^m=3$	-3.153 (0.181)
Constant	-1.484 (0.251)		

Prob. of public sector: $\Pr\left\{\mathrm{pub}_{it}=0 \mid e_{it}=1, e_{i,t-1}, \mathrm{pub}_{i,t-1}, z^v_{i,t-1}, z^f_i, k^m_i\right\},\ t \geq 2$			
Experience (years/10)	-1.815 (0.739)	Experience^2 (years^2/100)	0.154 (0.198)
High Education	0.958 (0.340)	Medium Education	0.618 (0.321)
Public last period: $\mathrm{pub}_{i,t-1}=1$	6.229 (0.820)	Public last period $\times$ Experience	2.975 (1.023)
Public last period $\times$ Experience^2	-0.430 (0.261)	Unempl. last period: $e_{i,t-1}=0$	2.390 (0.659)
Unempl. last period $\times$ Experience	0.348 (1.110)	Unempl. last period $\times$ Experience^2	0.106 (0.327)
$k^m=2$	1.709 (0.438)	$k^m=3$	-0.761 (0.353)
Constant	-3.848 (0.588)		

Table C.13: France: Parameters of job sector mobility (logit models), subsequent years

Earnings means: $\mu\left(\text{pub}_{it}, e_{i,t-1}, z^v_{it}, z^f_i, k^y_i\right)$			
High education	0.684 (0.005)	Medium education	0.166 (0.004)
Experience (years/10)	0.411 (0.010)	Experience^2 (years^2/100)	−0.071 (0.002)
Public: $\text{pub}_{it} = 1$	0.049 (0.016)	Experience×Public	−0.018 (0.015)
Experience^2×Public	0.012 (0.003)	$\text{Unemployed}_{i,t-1}$	−0.192 (0.012)
$k^y = 2$	0.022 (0.013)	$(k^y = 2)$ ×Experience	−0.041 (0.013)
$(k^y = 2)$ ×Experience^2	0.017 (0.003)	$(k^y = 2)$ ×Public	−0.071 (0.007)
Constant	8.687 (0.010)		
Earnings standard deviations: $\sigma\left(\text{pub}_{it}, e_{i,t-1}, z^v_{it}, k^y_i\right)$			
Experience (years/10)	0.083 (0.020)	Public: $\text{pub}_{it} = 1$	−0.158 (0.045)
$k^y = 2$	−0.155 (0.039)	$\text{Unemployed}_{i,t-1}$	0.080 (0.142)
Constant	−3.623 (0.052)		

Table C.14: France: Parameters of cross-sectional earnings means and standard deviations

First-order earnings autocorrelation: $\tau_1\left(\text{pub}_{it}, \text{pub}_{i,t-1}, z^v_{it}, k^y_i\right)$			
$(k^y = 1)$ ×(High Education)	−0.346 (0.052)	$(k^y = 1)$ ×(Medium Education)	1.694 (0.055)
$(k^y = 1)$ ×Public	0.402 (0.305)	$(k^y = 1)$ ×(Public last period)	−0.689 (0.305)
$(k^y = 1)$ ×(Experience (years/10))	−0.387 (0.023)	$k^y = 1$	−2.805 (0.068)
$(k^y = 2)$ ×(High Education)	−0.159 (0.073)	$(k^y = 2)$ ×(Medium Education)	−2.035 (0.062)
$(k^y = 2)$ ×Public	−0.364 (0.259)	$(k^y = 2)$ ×(Public last period)	0.089 (0.259)
$(k^y = 2)$ ×(Experience (years/10))	−0.318 (0.021)	$k^y = 2$	−1.061 (0.077)
Second-order earnings autocorrelation: $\widetilde{\tau}_2\left(k^y_i\right)$			
$k^y = 1$	−0.655 (0.034)	$k^y = 2$	−0.590 (0.033)

Table C.15: France: Parameters of earnings mobility

Mobility heterogeneity: $\Pr\left\{k_i^m = 2 \mid z_i^f\right\}$			
Experience (years/10)	2.175 (0.109)	High Education	0.759 (0.217)
Medium education	0.900 (0.189)	Constant	−3.092 (0.221)

Mobility heterogeneity: $\Pr\left\{k_i^m = 3 \mid z_i^f\right\}$			
Experience (years/10)	1.393 (0.098)	High Education	1.281 (0.188)
Medium education	1.122 (0.171)	Constant	−1.226 (0.179)

Earnings heterogeneity: $\Pr\left\{k_i^y = 2 \mid k_i^m, z_i^f\right\}$			
Experience (years/10)	0.023 (0.051)	High Education	0.367 (0.124)
Medium education	1.450 (0.110)	$k^m = 2$	−0.589 (0.164)
$k^m = 3$	−0.744 (0.143)	Constant	−0.133 (0.145)

Table C.16: France: Parameters of unobserved heterogeneity (multinomial logit models)

C.3.4 Italy

Initial unemployment probability: $\Pr\left\{e_{i1} = 0 \mid z_i^f, k_i^m\right\}$			
Experience (years/10)	−4.672 (0.289)	Experience^2 (years^2/100)	0.765 (0.070)
High education	−0.904 (0.275)	Medium education	−0.328 (0.166)
$k^m = 2$	1.895 (0.192)	$k^m = 3$	4.910 (0.266)
Constant	0.247 (0.227)		

Initial probability of public sector: $\Pr\left\{\text{pub}_{i1} = 1 \mid e_{i1} = 1, z_i^f, k_i^m\right\}$			
Experience (years/10)	1.579 (0.257)	Experience^2 (years^2/100)	−0.105 (0.057)
High education	3.296 (0.268)	Medium education	1.970 (0.156)
$k^m = 2$	4.612 (0.186)	$k^m = 3$	3.021 (0.198)
Constant	−7.219 (0.373)		

Table C.17: Italy: Parameters of job sector mobility (logit models), initial conditions

Unemployment prob.: $\Pr\left\{e_{it}=0 \mid e_{i,t-1}, \text{pub}_{i,t-1}, z^v_{i,t-1}, z^f_i, k^m_i\right\}$, $t \geq 2$			
Experience (years/10)	-3.510 (0.281)	Experience^2 (years^2/100)	0.592 (0.061)
High Education	-1.250 (0.197)	Medium Education	-0.299 (0.106)
Public last period: $\text{pub}_{i,t-1}=1$	-0.967 (0.461)	Public last period × Experience	-0.132 (0.532)
Public last period × Experience^2	-0.072 (0.121)	Unempl. last period: $e_{i,t-1}=0$	1.973 (0.283)
Unempl. last period × Experience	0.380 (0.356)	Unempl. last period × Experience^2	-0.047 (0.083)
$k^m=2$	2.674 (0.182)	$k^m=3$	5.292 (0.209)
Constant	-1.989 (0.273)		

Prob. of public sector: $\Pr\left\{\text{pub}_{it}=0 \mid e_{it}=1, e_{i,t-1}, \text{pub}_{i,t-1}, z^v_{i,t-1}, z^f_i, k^m_i\right\}$, $t \geq 2$			
Experience (years/10)	-0.241 (0.312)	Experience^2 (years^2/100)	0.074 (0.068)
High Education	1.009 (0.178)	Medium Education	0.677 (0.114)
Public last period: $\text{pub}_{i,t-1}=1$	4.235 (0.434)	Public last period × Experience	1.361 (0.413)
Public last period × Experience^2	-0.216 (0.088)	Unempl. last period: $e_{i,t-1}=0$	1.736 (0.450)
Unempl. last period × Experience	-0.353 (0.575)	Unempl. last period × Experience^2	0.096 (0.142)
$k^m=2$	1.699 (0.115)	$k^m=3$	1.463 (0.166)
Constant	-4.831 (0.336)		

Table C.18: Italy: Parameters of job sector mobility (logit models), subsequent years

Earnings means: $\mu\left(\text{pub}_{it}, e_{i,t-1}, z_{it}^v, z_i^f, k_i^y\right)$			
High education	0.527 (0.004)	Medium education	0.146 (0.002)
Experience (years/10)	0.313 (0.007)	Experience^2 (years^2/100)	-0.056 (0.001)
Public: $\text{pub}_{it} = 1$	0.214 (0.010)	Experience×Public	-0.180 (0.008)
Experience^2×Public	0.032 (0.002)	$\text{Unemployed}_{i,t-1}$	-0.102 (0.005)
$k^y = 2$	-0.016 (0.009)	$(k^y = 2)$ ×Experience	-0.059 (0.008)
$(k^y = 2)$ ×Experience^2	0.017 (0.002)	$(k^y = 2)$ ×Public	-0.019 (0.004)
Constant	7.515 (0.008)		
Earnings standard deviations: $\sigma\left(\text{pub}_{it}, e_{i,t-1}, z_{it}^v, k_i^y\right)$			
Experience (years/10)	-0.067 (0.018)	Public: $\text{pub}_{it} = 1$	-0.009 (0.039)
$k^y = 2$	-0.685 (0.037)	$\text{Unemployed}_{i,t-1}$	0.107 (0.099)
Constant	-3.663 (0.053)		

Table C.19: Italy: Parameters of cross-sectional earnings means and standard deviations

First-order earnings autocorrelation: $\tau_1\left(\text{pub}_{it}, \text{pub}_{i,t-1}, z_{it}^v, k_i^y\right)$			
$(k^y = 1)$ ×(High Education)	-0.160 (0.073)	$(k^y = 1)$ ×(Medium Education)	-0.252 (0.055)
$(k^y = 1)$ ×Public	-0.292 (0.111)	$(k^y = 1)$ ×(Public last period)	0.005 (0.112)
$(k^y = 1)$ ×(Experience (years/10))	-0.194 (0.024)	$k^y = 1$	-1.170 (0.085)
$(k^y = 2)$ ×(High Education)	-1.056 (0.082)	$(k^y = 2)$ ×(Medium Education)	-0.398 (0.039)
$(k^y = 2)$ ×Public	0.466 (0.098)	$(k^y = 2)$ ×(Public last period)	-0.594 (0.098)
$(k^y = 2)$ ×(Experience (years/10))	-0.447 (0.020)	$k^y = 2$	-1.527 (0.053)
Second-order earnings autocorrelation: $\widetilde{\tau}_2\left(k_i^y\right)$			
$k^y = 1$	-0.553 (0.034)	$k^y = 2$	-0.531 (0.030)

Table C.20: Italy: Parameters of earnings mobility

Mobility heterogeneity: $\Pr\left\{k_i^m = 2 \mid z_i^f\right\}$			
Experience (years/10)	-0.167 (0.041)	High Education	0.430 (0.156)
Medium education	-0.138 (0.092)	Constant	-0.030 (0.106)

Mobility heterogeneity: $\Pr\left\{k_i^m = 3 \mid z_i^f\right\}$			
Experience (years/10)	0.036 (0.051)	High Education	-0.215 (0.211)
Medium education	-0.734 (0.117)	Constant	-0.814 (0.134)

Earnings heterogeneity: $\Pr\left\{k_i^y = 2 \mid k_i^m, z_i^f\right\}$			
Experience (years/10)	-0.634 (0.042)	High Education	-2.139 (0.167)
Medium education	-0.900 (0.095)	$k^m = 2$	0.484 (0.094)
$k^m = 3$	-1.189 (0.123)	Constant	2.086 (0.128)

Table C.21: Italy: Parameters of unobserved heterogeneity (multinomial logit models)

C.3.5 Spain

Initial unemployment probability: $\Pr\left\{e_{i1} = 0 \mid z_i^f, k_i^m\right\}$			
Experience (years/10)	-2.614 (0.214)	Experience^2 (years^2/100)	0.387 (0.050)
High education	-0.484 (0.160)	Medium education	-0.127 (0.163)
$k^m = 2$	4.071 (0.293)	$k^m = 3$	0.869 (0.272)
Constant	-0.593 (0.316)		

Initial probability of public sector: $\Pr\left\{\text{pub}_{i1} = 1 \mid e_{i1} = 1, z_i^f, k_i^m\right\}$			
Experience (years/10)	1.680 (0.547)	Experience^2 (years^2/100)	-0.358 (0.120)
High education	-0.387 (0.392)	Medium education	-1.176 (0.470)
$k^m = 2$	-6.305 (0.420)	$k^m = 3$	-8.810 (0.489)
Constant	2.464 (0.625)		

Table C.22: Spain: Parameters of job sector mobility (logit models), initial conditions

Unemployment prob.: $\Pr\left\{e_{it}=0 \mid e_{i,t-1}, \text{pub}_{i,t-1}, z^v_{i,t-1}, z^f_i, k^m_i\right\}$, $t \geq 2$			
Experience (years/10)	-2.161 (0.159)	Experience^2 (years^2/100)	0.336 (0.033)
High Education	-0.677 (0.087)	Medium Education	-0.112 (0.086)
Public last period: $\text{pub}_{i,t-1}=1$	0.568 (0.449)	Public last period × Experience	-0.863 (0.439)
Public last period × Experience^2	0.197 (0.092)	Unempl. last period: $e_{i,t-1}=0$	1.406 (0.210)
Unempl. last period × Experience	-0.025 (0.229)	Unempl. last period × Experience^2	0.063 (0.051)
$k^m=2$	3.201 (0.195)	$k^m=3$	0.573 (0.199)
Constant	-1.252 (0.258)		

Prob. of public sector: $\Pr\left\{\text{pub}_{it}=0 \mid e_{it}=1, e_{i,t-1}, \text{pub}_{i,t-1}, z^v_{i,t-1}, z^f_i, k^m_i\right\}$, $t \geq 2$			
Experience (years/10)	0.113 (0.328)	Experience^2 (years^2/100)	-0.054 (0.069)
High Education	0.056 (0.132)	Medium Education	-0.140 (0.149)
Public last period: $\text{pub}_{i,t-1}=1$	3.718 (0.511)	Public last period × Experience	0.409 (0.462)
Public last period × Experience^2	-0.062 (0.094)	Unempl. last period: $e_{i,t-1}=0$	2.007 (0.487)
Unempl. last period × Experience	-0.672 (0.507)	Unempl. last period × Experience^2	0.167 (0.112)
$k^m=2$	-2.063 (0.150)	$k^m=3$	-3.607 (0.146)
Constant	-1.146 (0.376)		

Table C.23: Spain: Parameters of job sector mobility (logit models), subsequent years

Earnings means: $\mu\left(\text{pub}_{it}, e_{i,t-1}, z_{it}^{v}, z_{i}^{f}, k_{i}^{y}\right)$			
High education	0.483 (0.004)	Medium education	0.198 (0.005)
Experience (years/10)	0.554 (0.011)	Experience^2 (years^2/100)	−0.085 (0.002)
Public: $\text{pub}_{it} = 1$	0.297 (0.019)	Experience×Public	−0.177 (0.016)
Experience^2×Public	0.030 (0.003)	Unemployed$_{i,t-1}$	−0.207 (0.007)
$k^y = 2$	0.323 (0.014)	$(k^y = 2)$ ×Experience	−0.300 (0.013)
$(k^y = 2)$ ×Experience^2	0.050 (0.003)	$(k^y = 2)$ ×Public	0.007 (0.008)
Constant	11.386 (0.013)		
Earnings standard deviations: $\sigma\left(\text{pub}_{it}, e_{i,t-1}, z_{it}^{v}, k_{i}^{y}\right)$			
Experience (years/10)	0.038 (0.018)	Public: $\text{pub}_{it} = 1$	−0.130 (0.045)
$k^y = 2$	−0.116 (0.037)	Unemployed$_{i,t-1}$	−0.258 (0.077)
Constant	−3.414 (0.052)		

Table C.24: Spain: Parameters of cross-sectional earnings means and standard deviations

First-order earnings autocorrelation: $\tau_1\left(\text{pub}_{it}, \text{pub}_{i,t-1}, z_{it}^{v}, k_{i}^{y}\right)$			
$(k^y = 1)$ ×(High Education)	0.164 (0.049)	$(k^y = 1)$ ×(Medium Education)	0.173 (0.054)
$(k^y = 1)$ ×Public	−0.082 (0.106)	$(k^y = 1)$ ×(Public last period)	−0.032 (0.106)
$(k^y = 1)$ ×(Experience (years/10))	0.010 (0.020)	$k^y = 1$	−3.234 (0.067)
$(k^y = 2)$ ×(High Education)	−0.008 (0.054)	$(k^y = 2)$ ×(Medium Education)	−0.025 (0.064)
$(k^y = 2)$ ×Public	0.005 (0.097)	$(k^y = 2)$ ×(Public last period)	−0.090 (0.096)
$(k^y = 2)$ ×(Experience (years/10))	−0.129 (0.021)	$k^y = 2$	−1.413 (0.062)
Second-order earnings autocorrelation: $\widetilde{\tau}_2\left(k_{i}^{y}\right)$			
$k^y = 1$	−0.839 (0.030)	$k^y = 2$	−0.517 (0.036)

Table C.25: Spain: Parameters of earnings mobility

Mobility heterogeneity: $\Pr\left\{k_i^m = 2 \mid z_i^f\right\}$			
Experience (years/10)	-0.099 (0.059)	High Education	-2.083 (0.164)
Medium education	-1.504 (0.183)	Constant	1.474 (0.194)

Mobility heterogeneity: $\Pr\left\{k_i^m = 3 \mid z_i^f\right\}$			
Experience (years/10)	-0.519 (0.052)	High Education	-2.139 (0.145)
Medium education	-1.266 (0.158)	Constant	3.241 (0.172)

Earnings heterogeneity: $\Pr\left\{k_i^y = 2 \mid k_i^m, z_i^f\right\}$			
Experience (years/10)	-0.808 (0.052)	High Education	-1.154 (0.126)
Medium education	-0.997 (0.132)	$k^m = 2$	14.183 (29.157)
$k^m = 3$	0.311 (0.121)	Constant	1.539 (0.182)

Table C.26: Spain: Parameters of unobserved heterogeneity (multinomial logit models)

C.3.6 Portugal

Initial unemployment probability: $\Pr\left\{e_{i1} = 0 \mid z_i^f, k_i^m\right\}$			
Experience (years/10)	-2.483 (0.277)	Experience^2 (years^2/100)	0.404 (0.066)
High education	-0.604 (0.308)	Medium education	0.368 (0.206)
$k^m = 2$	-0.580 (0.211)	$k^m = 3$	2.453 (0.271)
Constant	-0.504 (0.251)		

Initial probability of public sector: $\Pr\left\{\text{pub}_{i1} = 1 \mid e_{i1} = 1, z_i^f, k_i^m\right\}$			
Experience (years/10)	2.690 (0.361)	Experience^2 (years^2/100)	-0.424 (0.087)
High education	2.006 (0.358)	Medium education	0.984 (0.317)
$k^m = 2$	-20.459 (360.806)	$k^m = 3$	-5.042 (0.348)
Constant	-1.590 (0.315)		

Table C.27: Portugal: Parameters of job sector mobility (logit models), initial conditions

Unemployment prob.: $\Pr\left\{e_{it}=0 \mid e_{i,t-1}, \mathrm{pub}_{i,t-1}, z^v_{i,t-1}, z^f_i, k^m_i\right\},\ t \geq 2$			
Experience (years/10)	-2.042 (0.236)	Experience^2 (years^2/100)	0.321 (0.046)
High Education	-1.422 (0.252)	Medium Education	-0.314 (0.153)
Public last period: $\mathrm{pub}_{i,t-1}=1$	0.997 (0.575)	Public last period × Experience	-1.132 (0.661)
Public last period × Experience^2	0.170 (0.146)	Unempl. last period: $e_{i,t-1}=0$	2.876 (0.305)
Unempl. last period × Experience	-0.138 (0.347)	Unempl. last period × Experience^2	0.027 (0.074)
$k^m=2$	0.156 (0.202)	$k^m=3$	3.469 (0.228)
Constant	-2.266 (0.287)		

Prob. of public sector: $\Pr\left\{\mathrm{pub}_{it}=0 \mid e_{it}=1, e_{i,t-1}, \mathrm{pub}_{i,t-1}, z^v_{i,t-1}, z^f_i, k^m_i\right\},\ t \geq 2$			
Experience (years/10)	0.494 (0.236)	Experience^2 (years^2/100)	-0.063 (0.049)
High Education	0.551 (0.151)	Medium Education	0.562 (0.132)
Public last period: $\mathrm{pub}_{i,t-1}=1$	2.601 (0.335)	Public last period × Experience	0.931 (0.326)
Public last period × Experience^2	-0.158 (0.068)	Unempl. last period: $e_{i,t-1}=0$	2.117 (0.463)
Unempl. last period × Experience	-0.328 (0.601)	Unempl. last period × Experience^2	-0.011 (0.144)
$k^m=2$	-3.435 (0.135)	$k^m=3$	-1.983 (0.151)
Constant	-2.157 (0.246)		

Table C.28: Portugal: Parameters of job sector mobility (logit models), subsequent years

Earnings means: $\mu\left(\text{pub}_{it}, e_{i,t-1}, z_{it}^{v}, z_i^{f}, k_i^{y}\right)$			
High education	1.004 (0.006)	Medium education	0.347 (0.004)
Experience (years/10)	0.342 (0.008)	Experience^2 (years^2/100)	-0.061 (0.002)
Public: $\text{pub}_{it} = 1$	-0.048 (0.016)	Experience$\times$Public	0.130 (0.014)
Experience^2$\times$Public	-0.017 (0.003)	Unemployed$_{i,t-1}$	-0.132 (0.010)
$k^y = 2$	0.072 (0.010)	$(k^y = 2)$ $\times$Experience	-0.018 (0.010)
$(k^y = 2)$ $\times$Experience^2	0.002 (0.002)	$(k^y = 2)$ $\times$Public	-0.068 (0.008)
Constant	11.254 (0.008)		

Earnings standard deviations: $\sigma\left(\text{pub}_{it}, e_{i,t-1}, z_{it}^{v}, k_i^{y}\right)$			
Experience (years/10)	0.057 (0.017)	Public: $\text{pub}_{it} = 1$	0.490 (0.048)
$k^y = 2$	0.401 (0.039)	Unemployed$_{i,t-1}$	0.024 (0.123)
Constant	-4.060 (0.045)		

Table C.29: Portugal: Parameters of cross-sectional earnings means and standard deviations

First-order earnings autocorrelation: $\tau_1\left(\text{pub}_{it}, \text{pub}_{i,t-1}, z_{it}^{v}, k_i^{y}\right)$			
$(k^y = 1)$ $\times$(High Education)	2.802 (0.082)	$(k^y = 1)$ $\times$(Medium Education)	2.332 (0.058)
$(k^y = 1)$ $\times$Public	-0.431 (0.081)	$(k^y = 1)$ $\times$(Public last period)	-0.105 (0.082)
$(k^y = 1)$ $\times$(Experience (years/10))	-0.366 (0.018)	$k^y = 1$	-3.299 (0.046)
$(k^y = 2)$ $\times$(High Education)	-1.357 (0.071)	$(k^y = 2)$ $\times$(Medium Education)	-1.774 (0.057)
$(k^y = 2)$ $\times$Public	-0.516 (0.071)	$(k^y = 2)$ $\times$(Public last period)	-0.223 (0.072)
$(k^y = 2)$ $\times$(Experience (years/10))	-0.301 (0.018)	$k^y = 2$	-1.586 (0.051)

Second-order earnings autocorrelation: $\widetilde{\tau}_2\left(k_i^{y}\right)$			
$k^y = 1$	-0.248 (0.036)	$k^y = 2$	-0.308 (0.037)

Table C.30: Portugal: Parameters of earnings mobility

Mobility heterogeneity: $\Pr\left\{k_i^m = 2 \mid z_i^f\right\}$			
Experience (years/10)	-0.283 (0.046)	High Education	-1.986 (0.193)
Medium education	-0.804 (0.136)	Constant	1.687 (0.110)

Mobility heterogeneity: $\Pr\left\{k_i^m = 3 \mid z_i^f\right\}$			
Experience (years/10)	0.492 (0.068)	High Education	0.012 (0.233)
Medium education	-0.564 (0.240)	Constant	-1.729 (0.192)

Earnings heterogeneity: $\Pr\left\{k_i^y = 2 \mid k_i^m, z_i^f\right\}$			
Experience (years/10)	0.089 (0.040)	High Education	0.439 (0.177)
Medium education	0.030 (0.123)	$k^m = 2$	-0.079 (0.103)
$k^m = 3$	1.188 (0.176)	Constant	-0.092 (0.123)

Table C.31: Portugal: Parameters of unobserved heterogeneity (multinomial logit models)

C.4 The EM Algorithm

In this appendix we fully detail the estimation procedure using the Expectation Maximization (EM) algorithm.

C.4.1 General Description

From Appendix C.2 and the model description in Section (4.4) the set of parameters to be estimated can be divided into two sub-sets: the parameters relating to labour market mobility process, denoted by Θ^m, and the parameters relating to the earnings mobility process, Θ^y. The mobility process parameters are $\Theta^m = \left\{(\kappa_k^m)_{k=1}^{K^m}, \psi_0, \chi_0; \psi, \chi\right\}$ and includes all the parameters involved in equations (C.21), (C.24) and (C.25). The earnings process parameters are $\Theta^y = \left\{(\kappa_k^y)_{k=1}^{K^y}, \mu(\cdot), \sigma(\cdot), \tau_1(\cdot), \widetilde{\tau}_2(\cdot)\right\}$, where in $\{\mu(\cdot), \sigma(\cdot), \tau_1(\cdot), \widetilde{\tau}_2(\cdot)\}$ we summarise all of the parameters of the corresponding functions, see equations (C.26), (C.27), (C.28) and (C.29).

The structure of (4.1) allows the individual contributions to the complete likelihood to be decomposed as $\mathcal{L}_i(\mathbf{x}_i, k_i; \Theta^m, \Theta^y) = \mathcal{L}_i^m(\mathbf{x}_i, k_i^m; \Theta^m) \cdot \mathcal{L}_i^y(\mathbf{x}_i, k_i^m, k_i^y; \Theta^y)$, where:

$$\mathcal{L}_i^m(\mathbf{x}_i, k_i^m; \Theta^m) = \ell_i\left(\mathbf{e}_i, \mathbf{pub}_i \mid \mathbf{z}_i^v, z_i^f, k_i^m; \Theta^m\right) \cdot \Pr\left\{k_i^m \mid z_i^f; \Theta^m\right\} \tag{C.41}$$

Here the dependence of the various parts of the likelihood on the sets of parameters is made explicit. This separability makes it possible to integrate earnings sequences ($\mathbf{y}_i$) and wage classes (k_i^y) out of the complete likelihood $\mathcal{L}_i(\mathbf{x}_i, k_i; \Theta^m, \Theta^y)$. This allows us to recover the parameters relating to the labour market mobility process and the mobility classes by maximising the log-likelihood of the observed job sector mobility, $\sum_{i=1}^N \log\left(\sum_{k_i^m=1}^{K^m} \mathcal{L}_i^m(\mathbf{x}_i, k_i^m; \Theta^m)\right)$. This maximisation can be achieved by a straightforward application of the EM algorithm for finite mixtures, described below (in sub-section: Estimation of the Job Mobility Parameters Θ^m).

This first stage produces estimates of the labour market mobility parameters, which we fix at their estimated values, $\hat{\Theta}^m$, and return to the maximisation of the sample log-likelihood, $\mathcal{L}_i\left(\mathbf{x}_i, k_i; \hat{\Theta}^m, \Theta^y\right)$ but now switching attention to the earnings process component of the likelihood, $\mathcal{L}_i^y(\mathbf{x}_i, k_i^m, k_i^y; \Theta^y)$, and the relevant parameters Θ^y. Details of this part of the estimation are below (see sub-section: Estimation of the Earnings Parameters Θ^y).

Estimation of the Job Mobility Parameters Θ^m

The standard EM-algorithm involves iterating two steps: the **Expectation** step and the **Maximization** step:

E-step: For an arbitrary initial value Θ_n^m of Θ^m, for each mobility class index $k^m = 1, \ldots, K^m$, and for each individual i in the sample (for that country), compute the posterior probability that i belongs to mobility class k^m given $\mathbf{x}_i$ and Θ_n^m:

$$\Pr\left\{k_i^m = k^m \mid \mathbf{x}_i; \Theta_n^m\right\} = \frac{\mathcal{L}_i^m(\mathbf{x}_i, k_i^m; \Theta_n^m)}{\sum_{k=1}^{K^m} \mathcal{L}_i^m(\mathbf{x}_i, k_i^m; \Theta_n^m)} \tag{C.42}$$

M-step: Update Θ_n^m to Θ_{n+1}^m by maximizing the following augmented sample log-likelihood, weighted by C.42:

$$\Theta_{n+1}^m = \arg\max_{\Theta^m} \sum_{i=1}^{N} \sum_{k=1}^{K^m} \Pr\left\{k_i^m = k \mid \mathbf{x}_i; \Theta_n^m\right\} \cdot \log\left[\mathcal{L}_i^m(\mathbf{x}_i, k_i; \Theta^m)\right] \tag{C.43}$$

This maximization can be straightforwardly executed by running separate weighted logit regressions for ψ and χ (the parameters relating to the employment sector equations, (C.24) and (C.25)) and a separate weighted multinomial logit for the class weight parameters κ_k^m, using (C.42) as the weights in each case. In theory this algorithm converges to the maximum-likelihood estimator of Θ^m (see Dempster *et al.*, 1977); in practice, we stop iterating when the maximum relative change in any of the parameters in Θ^m from one iteration to the next falls below 10^{-3} (i.e. less than a 0.1% change in any parameter between successive iterations). At this point we have our estimate of $\hat{\Theta}^m$.

Estimation of the Earnings Parameters Θ^y

The next stage is to estimate the second subset of parameters, those which relate to earnings, Θ^y. The natural approach at this stage would be to maximize the sample likelihood, $\mathcal{L}_i\left(\mathbf{x}_i, k_i; \hat{\Theta}^m, \Theta^y\right)$, with Θ^m fixed at its estimated value from the first stage of the estimation procedure, $\hat{\Theta}^m$. However, given the highly non-linear nature of $\mathcal{L}_i^y(\cdot)$ — see subsection 4.4.4 — even this maximisation is numerically impractical. Thus at this point we use a sequential, limited information, version of the EM algorithm. The procedure is as follows:

E-Step: For an arbitrary initial value Θ_n^y of Θ^y, for each class index $k = (k^m, k^y)$, with $k^m = 1, \ldots, K^m$, $k^y = 1, \ldots, K^y$, and for each individual i in the sample (for that country), compute the posterior probability that i belongs to mobility class k^m and wage class k^y given $\mathbf{x}_i$, Θ_n^y and $\hat{\Theta}^m$:

$$\Pr\left\{k_i^m = k^m, k_i^y = k^y \mid \mathbf{x}_i; \hat{\Theta}^m, \Theta_n^y\right\} = \frac{\mathcal{L}_i(\mathbf{x}_i, k^m, k^y; \hat{\Theta}^m, \Theta_n^y)}{\sum_{k^m=1}^{K^m} \sum_{k^y=1}^{K^y} \mathcal{L}_i(\mathbf{x}_i, k^m, k^y; \hat{\Theta}^m, \Theta_n^y)} \tag{C.44}$$

M-step: This is the point at which our algorithm differs slightly from the standard EM algorithm. We proceed as follows:

1. Update income mean parameters $\mu(\cdot)$ using a weighted OLS regression of y_{it} on $\left(\text{pub}_{it}, z_{it}^v, z_i^f, k_i^y\right)$, using (C.44) as weights. Denote the updated function $\mu(\cdot)$ as $\widehat{\mu}_{n+1}(\cdot)$.

2. Take the log of the squared residuals from the latter regression and perform a weighted OLS regression of those on $\left(\text{pub}_{it}, z_{it}^v, z_i^f, k_i^y\right)$, again using (C.44) as the weights, to update the variance parameters $\sigma(\cdot)$. Denote the updated function as $\widehat{\sigma}_{n+1}(\cdot)$.

3. Form the log earnings disturbances $\tilde{y}_{it}^{(n+1)} = \frac{y_{it} - \widehat{\mu}_{n+1}\left(\text{pub}_{it}, z_{it}^{v}, z_{it}^{f}, k_i^y\right)}{\widehat{\sigma}_{n+1}\left(\text{pub}_{it}, z_{it}^{v}, z_{it}^{f}, k_i^y\right)}$. Update $\tau_1(\cdot)$ as:

$$\widehat{\tau}_{1,n+1}\left(\text{pub}_{it}, \text{pub}_{i,t-1}, z_{it}^{v}, k_i^y\right) = \text{cov}\left(\tilde{y}_{it}^{(n+1)}, \tilde{y}_{i,t-1}^{(n+1)}\right), \tag{C.45}$$

given that $\left(\tilde{y}_{it}^{(n+1)}, \tilde{y}_{i,t-1}^{(n+1)}\right)$ is distributed bivariate normal with unit variances. We do this by weighted maximum likelihood, again using (C.44) as weights. Then we similarly update $\tilde{\tau}_2(\cdot)$ knowing that $\tau_2(\cdot) = \left(\tilde{y}_{it}^{(n+1)}, \tilde{y}_{i,t-2}^{(n+1)}\right)$ is given by the formula (C.29), and that $\left(\tilde{y}_{it}^{(n+1)}, \tilde{y}_{i,t-2}^{(n+1)}\right)$ is again distributed bivariate normal with unit variances. Note that $\tau_1(\cdot)$ is involved in (C.29) and that we replace it by $\widehat{\tau}_{1,n+1}$ for an update of $\tilde{\tau}_2(\cdot)$.

4. The final part is to update the set of earnings class assignment parameters, $(\kappa_k^y)_{k=1}^{K^y}$ by running a weighted multinomial logit regression of class indexes on $\left(z_i^f, k_i^m\right)$, again using (C.44) as the weights.

We iterate between these E- and M-steps until the maximum relative change in any of the parameters in Θ^y from one iteration to the next falls below 10^{-3} (i.e. less than a 0.1% change in any parameter between successive iterations).